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THE CONCEPT OF THE ORGANISM-AS-A-WHOLE AND ITS APPLICATION TO CLINICAL SITUATIONS*

BY HANS SYZ, M. D.

Research Associate, The Lifwynn Foundation, New York

THERE has been much discussion in recent years of the concept of the "organism-as-a-whole" and its application to normal and disordered behavior processes. Adolf Meyer (1) has been outstanding in promoting this idea in the field of psychiatry. Other investigators also have advocated this broader concept within the sphere of behavior problems. However, as frequently occurs where concepts begin to be generally accepted, the idea of the "organismic whole" has too often been used as a mere label, as a phrase that is not based on a real understanding of the biological structures and interrelations to which the concept refers. This tendency to use symbols or verbal concepts without due regard for the underlying structures constitutes, according to the investigations of Trigant Burrow (2), one of the major cultural problems of our time.

It may not be superfluous, therefore, to recall briefly the biological material from which the concept of the "organism-as-a-whole" has been derived. This will be followed by a discussion of the relation of this concept to the experimental investigations of human behavior and its

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disorders as introduced by Burrow. Finally, some of the applications to clinical situations will be outlined.

I

Schaxel (3) and a number of other biologists, for instance Bertalanffy (4) and Woodger (5), have shown that a great many conceptual prejudices have crept into biological research, for instance in the controversy between mechanism and vitalism which was based largely on preconceived ideas and on inconsistencies in the fundamental concepts. They pointed out that we know life primarily in terms of organismic units, of functioning systems which tend to maintain their own structure and balance, and that it is necessary to accept this "concept of the organism" as an essential principle in biological research—a principle as fundamental as the "concept of energy" in physics. Bio-chemists such as Donnan (6), Hill (7), and Meyerhof (8) have emphasized that the dynamic interdependence of the various organs and functional patterns in the living organism and their relation to the stabilization and self-perpetuation of the total system, represent fundamental problems which cannot be expressed directly in physico-chemical terms. The type of relatedness of an organ or part-function to the total system thus becomes a legitimate and important object of investigation; the partial patterns are to be viewed, morphologically and functionally, spatially and temporally, in relation to the structure and function of the organism as a whole.*

Much concrete evidence of the constitution of the organism as a total structure and its relation to behavior is found in the work of a few embryologists and zoologists, notably Child (14), Coghill (15) and Spemann (16). It is hardly necessary here to mention in detail Coghill's observations on the primarily integrated total pattern in which partial patterns develop by individuation. While observations of Windle (17) and Carmichael (18) seem to indicate that distinctive reflexes may be the first behavior reactions in higher vertebrates, the work of Gonzales (19) on rats and Minkowski (20) and Bersot (21) on human embryos

*The concept of the "whole" has been discussed by a number of authors, for instance Alexander (9), Lloyd Morgan (10) and Smuts (11). At times there has been a philosophical preconception in these formulations which impaired their validity; thus for example when Driesch (12) assumed a metaphysical totalizing force called "entelechy" or when J. S. Haldane (13) championed with undue fervor a somewhat vague theory of "organicism".

appears to substantiate Coghill's views of the primacy of the total reaction patterns. Child's work on functional gradients offers important information on the physiological foundations of organismic integration, and Spemann's experiments on "developmental organizers" give further evidence of the potency of organizing principles in the embryo's growth processes. Heidenhain's (22) morphological studies have shown how correlated and subordinated histological systems are integrated to form the total structure.

In human physiology Cannon's (23) work has been outstanding in demonstrating the subtle equilibration of the total organismic system in its interweaving of humoral and neural factors. From the neurological point of view Sherrington's (24) work meant an important impetus in the consideration of integrated function. The experimental studies of Magnus (25) and his co-workers on the interrelation of postural reflex patterns and the investigations of Hess (26) on the interaction between the vegetative and animal (cerebro-spinal) nervous systems are especially noteworthy contributions to the comprehension of the organism as a total unit. Goldstein's (27) analysis of neurological processes has added further concrete material which helps us to understand how in the organism's adjustment to enviromental changes sensory and motor part-functions are integrated in the total action patterns.

Naturally we think in this connection of the Gestalt psychologists, Koffka (28), Köhler (29) and Wertheimer (30), with their forerunners, von Ehrenfels (31) and Meinong (32). They state that perceptions constitute total patterned systems in which the whole is more than the sum of the parts (33). As Parsons (34) pointed out, these perceptual patterns represent psychological units which can be analyzed into constituent "sensations" only by a process of abstraction. He and other investigators, for instance Pieron (35) and Schilder (36) have shown that perception progresses from generalized, diffuse and vague impressions to more specific and differentiated patterns. Similar principles were found to apply to the formation of thought and language responses. Krueger (37) and his co-workers have emphasized the significance of the total configuration for an understanding specifically of emotional reactions. Gesell (38) in his investigations of the motor development has shown how new behavior patterns gradually differentiate, though they never completely individuate; they remain accessory to a fundamental unity of response. His observations lead to the conclusion that physical and mental development are basically unitary and

that the laws of maturation, that is, the differentiation of part systems under the dominance of the total system as established for motor patterns, may be applied also to the higher orders of thought and feeling.*

For our purpose those larger configurations which the organism forms with its environment are of special interest. As Herrick (40) says, "all life is interaction between a protoplasmic organization and the energies of an appropriate environment". The physiologist Lawrence Henderson (41), in his book on the "Fitness of the Environment", has discussed the close chemical interaction between living beings and the surrounding world. However, we are concerned here more with the behavior level. The work of von Uexküll (42) is especially important in this regard. His detailed observations on form and function in the lower metazoa show that every animal species is closely fitted into a particular environment; the internal structure and the structure of the environment form together a total organization which is interlinked by the prevailing behavior patterns.

From this biological background we may consider the social integration of living organisms as a variety or specific application of their general integration within the environment. The beginnings of social integration in animal aggregations have been reviewed by Allee (43) who has shown the various transitions from primitive synchronism of response, to those group formations in which animals are obviously and intimately interrelated in a social unit. On the whole we find established in the life of social animals, on a purely organic and non-conscious level, a dynamic configuration which tends to assure the perpetuation of the social group. Sex behavior is one important component in this interactive structure, but there are many other inherent social patterns, such as play activities, nest building, parental care, storing of food, association of individuals without reference to mating, socially defensive behavior—behavior patterns which are not of primarily sexual significance though they possess survival value for the individual and the group. W. M. Wheeler (44) in his description of insect societies speaks of superorganisms in which whole organisms function as interacting component

*We may mention at this point that some investigators have made the attempt to extend the concept of the organism to include all natural phenomena; for instance Whitehead (39) expressed the opinion that in the physical world generally things are interrelated and interbalanced similarly to the equilibration observed in living systems.

parts. Whatever may be the factors involved in evolution, the evidence indicates that the organism-environment unity in existing animal species —on the physico-chemical, behavioral and social levels—is a primary fact and not merely a secondary derivation.

As far as human society is concerned some investigators, for instance R. M. Ogden (45), H. Werner (46), R. H. Wheeler (47), have maintained that the individual is structurally and dynamically so much an integrated part of the surrounding social and cultural patterns that the investigation should begin with the behavior expression of the group and then proceed to the psychological and physiological manifestations of the individual. K. Lewin (48) has pointed out that behavior manifestations together with the environmental field should be viewed as a total dynamic structure. Similar views have been expressed in sociology for instance by Roberty (49) and Durkheim (49) and by other investigators who considered individual and social reaction patterns as a joint and interwoven configuration (Benedict (50), Cooley (51), Mead (52), Spann (53), Sumner and Keller (54)). While one should be careful in following Spencer's (55) suggestion of applying bio-organismic theories to society, most sociologists agree, as Sorokin (49) has said, that "human society represents a kind of system, a kind of a living unity different from the mere sum of the isolated individuals". There is the unique circumstance that in human society the environment represents a reflection and institutionalization of specifically human emotional-symbolic patterns and reactivities. So that some of the configurational principles which are evident in the structure and function of living organisms in general may also be applied to those social institutions, values and relations of which the individual's emotional and behavioral processes are an interwoven part.

Some of the essential features of organismic configuration have been set forth and emphasized by Adolf Meyer (1) as they relate to psychiatric problems. He pointed out how elements and part functions may combine to form novel and not fully predictable patterns which in the personality structure are coordinated at different levels of integration. Similar observations have led other investigators to the concepts of "emergent evolution" (C. Lloyd Morgan (10)) or "creative synthesis" (G. P. Conger (56)). Max Wertheimer (57) has laid stress on another significant aspect of configurational structures, namely, that they may maintain their essential qualities, even though there are changes among the component parts; and on the other hand, that a

change at a pivotal point may bring about far reaching alterations throughout the entire system.

II

In its application to behavior problems, the organismic concept helps us to understand and interrelate manifestations which otherwise would be difficult to comprehend in their interaction. It helps us to understand how manifestations that look different in their external aspects may nevertheless be not only dynamically interrelated but also structurally of similar or identical nature. But as mentioned before, the theory of the organism as derived from the various branches of biology is one thing and the actual experimentation with the organism is another.

In referring to experiments with human behavior processes I have in mind the laboratory investigations into human reaction-tendencies as introduced by Trigant Burrow (58). Focussing his observations on basic adaptive patterns, Burrow in his group- or phylo-analytic procedure investigated the interrelation of neurotic behavior tendencies with the surrounding normal action patterns, stressing in this inquiry specific deviations or incongruities observable in the interreactive structure of the immediate setting of the investigation.*

In any group consisting of normal and neurotic individuals—whether an experimental group or a group accidentally thrown together for some social or occupational purpose—the observable interreactions were found to embody throughout a specific maladaptive constellation. This quite general constellation may be designated as "organismic de-centration", "attentional deviation" or "adaptive deflection", because it relates not only to neurotic maladjustments but also to incongruities of normal behavior, to destructive and disorganizing social trends, and to a conflicting tensional configuration that underlies these behavior phenomena.

A prerequisite to entering upon a closer study of this problematic adaptive constellation is the realization on the part of the observer that he himself is part of the observed reaction-tissue. Without including

*The term "experimental" is used here to characterize an investigative situation which has met, as far as this is possible in the field of behavior reactions, the criteria of experimental procedure that prevail in other fields of investigation. The processes under examination were observed in a relatively constant and controlled setting in which certain pertinent factors were systematically varied and modified.

one's own personal equation it is apparently impossible to arrive at a comprehensive and objective evaluation of the behavior phenomena before us. A mere visualization of the "facts" as existing outside, without full inclusion of essentially identical patterns in one's own adaptive processes, must lead to an incomplete or distorted picture of the data before us. This experimental, self-inclusive technique cannot be substituted for by a mere intellectual or verbal acknowledgment of the self-involvement; it has nothing to do with an "I am too" attitude and cannot be established "intuitively" or through the intellectual refrain that "everybody of course has his little psychosis". Neither will analysis of specific fixations and developmental impediments in the psychoanalytic sense replace the definite inclusion of the normally systematized type of adaptive deflection. The most refined reasoning, knowledge or technique on the "projective" basis can not substitute for the practical realization on the observer's part that he is an active component of the observed reaction-tissue, embodying in his own organism essentially the same type of tensional imbalance whose expression he is witnessing in the environmental reactions about him.

This problem of self-inclusion was emphasized by Burrow (59) more than ten years ago when he referred to the need of considering more specifically the impediments to observation due to the investigator's personal equation. It is of interest in this connection that not only in biology has there been an increasing recognition of the distorting effect of emotional preconceptions but also in the development of modern physics a definite awareness of the observer's relative position has taken place; in quantum mechanics it has been recognized that the very tools and methods of observation modify the findings, and that this has to be taken into account in planning the research procedure.

Setting aside conventional distinctions and social pretenses, we find in the average (normal) group setting a network of interwoven reaction tendencies which may be characterized, on the symptomatic level and in non-technical language, as aggression, disguised competition and self-defense, as an emphasis on a socially approved appearance of behavior that is in contradiction to underlying egocentric trends. Self-deception and deception of others are closely associated aspects. A multitude of antisocial features relate to this moralistic assertion of the self-image which may appear in a proprietary, authoritarian attitude as well as in the apparent alternative of dependence. This whole reaction structure, as observable though not commonly observed in any group situation,

shows a close connection with the more acute indications of hostility, guilt, anxiety, substitution, projection and related mechanisms with which we deal in clinical disorders. Of course it is generally recognized that certain normal attitudes—such as parental overprotection, indifference or rejection—are of importance in the early genesis of behavior deviations. But we are interested here in the consistent and continuous interreaction of these normal and neurotic behavior trends, in the unfavorable influence of a specific type of social conditioning everywhere and observable also in the immediate experimental setting.

A familiarity with the concept of organismic configuration will help us to understand how behavior phenomena which on the surface appear distinct—the normal parental attitudes, sentimental dependence and authoritarian dominance, feelings of guilt, insecurity and anxiety, infantile temper tantrums, neurotic aggression and mechanisms of self-defense, the antisocial performance of the delinquent and the charitable overcompensation of the more acceptably adapted individual—how all such phenomena may be inter-dependent manifestations that belong to one central constellation. They are inter-related not only in one and the same individual but also in the interreactive structure which constitutes the social group. Naturally they appear different when focussed upon as isolated individual phenomena than when seen as part of an interacting structure. Organismic formulations help us to conceive of the possibility that relatively simple adaptive patterns may be at fault in these phenomena of maladjustment, individual and social. Instead of attempting to trace these reaction tendencies to fundamental instincts and their social repression (for instance sex, self-preservation, aggression), they are found to be related, as Burrow has pointed out, to a specific developmental impediment, to an organismic imbalance in which, viewed from a more comprehensive frame, projective symbol and image formation become unduly attached to emotional dispositions and then serve in a false or disproportionate manner as a point of reference or focus of equilibration.

In this connection we may be helped again by biological parallels observed in other organisms. Coghill (60) has shown in his work on the growth and behavior of *Amblystoma* that the organism functions from the beginning as a total unit, and he has suggested that the over-individuation or insubordination of the part may constitute the biological basis for conflict and behavior disorder in human beings. Similar views in the interpretation of neuro-pathological material have been expressed

by Goldstein (61) who emphasized that the lack of centration and the relative isolation of part-functions may be synonymous with pathological function and with a disturbed adaptive process.

Burrow (62) has considered in a more specific way the function of the organism-as-a-whole and the disturbance caused by the overindividuation of the part. A "partitive overemphasis" has to do in his interpretation with certain developmental peculiarities of the symbolic system. That is, the specifically human type of symbol utilization has come to occasion an adaptive imbalance in which total feeling reactions have become unduly attached to or displaced into the symbolic segment. There is with this in the organism's adaptive life a decentration in the direction of socially or personally conditioned image-formations. The organism's total reaction tendency and its direct adjustment to the objective world has been interfered with by a secondarily acquired system of motivation. The part function—symbol, word or gesture—which is invaluable in man's economy and in his management of things, is thus allowed to exert an undue influence upon the organism's total function and adaptation. In the experimental approach to this imbalance, as developed by Burrow in recent years, the emphasis is placed on "internal postural tensions and strains" connected with the attentional deflection and perceptible in the symbolic segment, particularly as they relate to the function of the eyes. Consistent observations have led to a discrimination between a tensional pattern that is de-centered towards the symbolic segment in contradistinction to a tensional state that is more evenly distributed throughout the total organism.

From this background we may arrive at a more unitary formulation of behavior and personality disorders. A large variety of behavior items, including a great deal of the clinical symptomatology, may be viewed from the frame of this common adaptive deflection. The basic configurational disorder may combine or merge with instinctual drives and thus lead to those instinct miscarriages, fixations and self-defeating vicious circles which we see in clinical reaction pictures. From such a basis we have to revise our views regarding so-called primary instincts, such as sex, self-preservation and aggression, as we know these behavior patterns only as expressions of an organismic constellation which has been modified by a specific application of the symbolic part function. The various phases and mechanisms of the neurotic maladaptation (past and present) are seen in the larger frame of this attentional deflection

which, with the concomitant efforts at organismic re-equilibration, is the object of further experimentation and research.

What appears of especial significance in Burrow's investigations is the circumstance that behavior phenomena are actually worked with experimentally in oneself and that the problem presents itself not in terms of images of behavior reactions or of emotionally toned projective phantasies, but as an intra-organismic condition which is back of the image, as it were, and which is appreciable in terms of tensional patterns. The organism-as-a-whole and the impediments to its total adaptation are approached *from within* in terms of perceptible postural configurations. With this direct approach to underlying postural patterns we touch a focus in the organismic system that is prior to the image-affect attachment, or lies beneath those reaction formations with which psychiatry and psychoanalysis customarily deal. That is, behavior processes are reached, as it were, in their *status nascendi,* before they have merged into their composite, projectively perceivable form.

That significant interconnections exist between tensional patterns and behavior is not surprising. Jacobson's (63) experimental studies show the close connection of motor activation with feeling and thinking. A number of other investigators (Holt (64), Langfeld (65), Washburn (66), Watson (67)) have in their "motor theory of consciousness" pointed out the importance of incipient motor reactions in the structure of "psychic phenomena". So that it becomes understandable how internally appreciable postural tensions relating to the symbolic segment may be integrated with specific attitudes or modes of behavior, with an adaptive peculiarity that is embodied in the complex which we call the "I " or "self".

The experimental evidence of the tremendous influence upon the individual of social reaction patterns corroborates the view expressed by Burrow that the adaptive constellation or imbalance to which we here refer is largely a matter of interindividual conditioning (68). Racially we may have to do with a temporary impediment or miscarriage of development but, from the standpoint of the individual, the situation may be more conveniently viewed as an inappropriate type of conditioning. This faulty or partitive constellation is seen to be in conflict with a more basic principle of biological coordination that prevails throughout the total system, internal to the organism as well as throughout the racial structure. The evidence of this trend towards coordinated total interaction in human function Burrow has referred to in his early work

under the term of the "preconscious phase" (69), and the observations on organismic function mentioned in the beginning of this paper give further evidence in this direction. In contradistinction to this organismic, total and coordinated mode of function our present form of social adaptation may rather be characterized, to use Burrow's term, as a "social neurosis" (70), for it contains an observable adaptive deflection throughout that culminates in behavior disorders, both individual and social. The indications are that if the problem of a specific adaptive misconstellation is further followed up experimentally, we may come to understand a great deal about our behavior disorders in relation to this misconstellation. In the measure in which we come in closer contact with this problem and with our efforts of readjustment at this pivotal point of disorganization, there may be brought about a change throughout the total pattern that constitutes a more unitary means of organismic recentering and re-coordination.

III

In applying to clinical situations the position here outlined there occur a number of changes, conceptually as well as in the actual relation to patients or students. Burrow discussed a number of years ago some of the problems of the group-analytic setting and the altered relationship it entails between the physician and the individual participants (71). As must be evident from the foregoing remarks, the immediate psychotherapeutic interest constitutes only one aspect of these researches which are carried on by The Lifwynn Foundation under the direction of Dr. Burrow, and which aim at a clearer understanding of fundamental issues in behavior disorders. In however limited a measure I have been able to introduce into my clinical work an altered attitude based on my experience in this socio-biological technique of experimentation, the response has been gratifying and has led to a few interesting observations.

From this background, the individual patient is not viewed in contradistinction to a social environment to which he is supposed to adapt; individual and society are not considered as separate units. Instead the neurotic patient is regarded as part of the social structure which embodies an adaptive deflection identical with that enacted by himself. Thus the patient is placed from the beginning in the role of a responsible student of his own maladjustment as it relates to a socially interwoven reaction-tissue. He is given the opportunity of sensing and observing

the inadequacy of his neurotic adaptation (in its early and recent phases) as part of an undeveloped or maladapted social configuration.

There is no tendency or interest on the part of the physician to impose theories of behavior or to urge any special insight. Neither does he adopt an artificially passive role. On the basis of the experimentation with his own reactions, the physician senses and considers the "neurotic" attitude evidenced by the patient as structurally identical with a mode of adaptation that is observable in its social extension everywhere and in attentional postural patterns within himself. From this basis it is possible for him to approach quite directly the material presented by the patient without causing resistance or increase of guilt and anxiety. Self-contradictions, evasions, projections and the whole range of phenomena relating to the moralistically over-accentuated self-image as presented in the patient's feeling and behavior and as commonly corroborated socially may be brought up for direct consideration. These behavior incongruities are understood not as isolated personal characteristics but as components in an interreactive structure that includes not only the patient and his family situation but also the psychiatrist and the larger social setting. Incidents of the same adaptive inadequacy or pathogenic constellation may be brought to observation as they occur also in the setting of the immediate therapeutic situation.

As a rule I follow up in the appointments with the patient the usual developmental and situational aspects. But the aggravating or conditioning features thus unravelled do not occupy the important position often given to them in the habitual interpretation. Rather they are considered together with the neurotic symptomatology as a secondary variation of a basic theme, as a specific application or expression of a configurational peculiarity that is evident and active throughout behavior reactions generally. Instead of collecting innumerable details of behavior reactions, the emphasis is placed on a few important aspects, on specific fundamental attitudes which are observable not only in the material presented by the patient but which are found to favor neurotic development throughout the social structure. The patient is not told what he should believe but he is merely given the opportunity of sensing and understanding the familiar events of his experience and behavior in a new frame of reference.

As experience has shown, there occur on the basis of such a procedure marked and lasting alterations of attitude, behavior symptomatology and personality organization; there occurs a shift in the direction

of a more organic or coordinated type of function, internally as well as inter-individually. Though there is little emphasis on a detailed analysis in terms of early experiences and fixations, a recentering of attention, a withdrawing of emotional emphasis from socially conditioned image patterns takes place which results in a behavioral rehabilitation. In how far special therapeutic attention has to be given to early conflicts and secondary resultants and in how far a reorientation and coordination may be reached by a more direct approach to physiological tensional patterns concomitant with the adaptive deflection, is a problem that awaits further clinical investigation.

It is interesting that in this procedure there is a definite modification of the problems relating to the transference situation. Observations which for the patient imply a far going reorientation of outlook and an insight into undesirable trends are not reacted against with the resentment or resistance that would ordinarly be expected. Neither is there any increase of guilt and anxiety; the development is rather towards a less moralistic outlook. The patient is not being confronted by an authority that imposes opinions or urges insight. In fact the authoritarian image of threat or approval which is implied in any normal situation (even though the psychiatrist may assume a "passive" or "objective" attitude), is counteracted from the beginning. It is included as one phase of the adaptive difficulty from which the patient is suffering and of which his tendency to depend is the alternative reaction. The goal is not merely to adjust distorted forms of emotional attachment and to shift the dependence from early fixations to more acceptable personality images. Whether the individual unites in a comfortable identification with a flattering personality image, or whether he tries to assert his pictorial identity in antagonism toward a disapproving image projection, the problem centres upon the entire constellation of these habitual forms of adjustment, with their domination-dependence alternation and their moralistic-possessive outlook, irrespective of its accidental expression.

Expressed in terms of a practical therapeutic situation, one does not find a transference of the usual type either in a positive or negative direction. The patient does not cling to the physician in a dependent or admiring way nor does he show marked resentment and resistance. At the same time there is no difficulty in keeping up the contact, though the appointments are not frequent (one or two a week). The patient is glad to use the physician as an impersonal agent for his own rehabilitation and to return for treatment as long as this is necessary.

From this basis the therapeutic contact may easily be extended to other members of the family. In children as well as in adults it is a definite asset from this extended background for the same physician to see also relatives of the patient in individual conferences as well as in joint meetings with the patient. There is no occasion for jealousy or competition, as the behavioral inadequacies are sensed and brought up as a socially interreactive misconstellation and not in the sense of a differentiation between the patient and his environment. In as far as the physician has not worked with these habitual patterns in himself he may unconsciously encourage resistance or jealousy reactions. But from the more inclusive basis a direct and comprehensive observation of basic adaptive incongruities is welcomed by those who compose the therapeutic setting.

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POTENTIAL AND ACTUAL REPRODUCTIVE PERFORMANCE IN MAN[1]

BY RAYMOND PEARL

I

IN CONSIDERING the general problem of human fertility and reproduction the simple question implied by the title of this paper naturally occurs at once to a biologist as basic. The particular physiological mechanism of reproduction sets a limit to the time rate of reproduction of any organism. How closely does actual reproductive performance approach this physiological limit in a particular species? In general it is known from observations in field and laboratory, as well as from breeding experience with domestic animals, that the approach is fairly close for most animals lower than man in the evolutionary scale. There are, of course, inherent individual variations in fecundity and fertility that lead to differences between individuals in expressed reproductive capacity. Also illness and advancing age may temporarily or permanently slow down the rate of reproduction in particular individuals. Furthermore complete physiological sterility may occur in lower animals as it does in man. But, with due allowance for the effects of such factors as these, it is the fact that in the normal way of life a lower animal such as a fly or a frog, for example, keeps on reproducing up to the limit of its capacity throughout the portion of its life span within which it is able physiologically to reproduce at all.

With man the case is somewhat different. Besides the physiological limitation of reproductive capacity in the female, which confines her child-bearing to the period between menarche and menopause, there are two other factors of major importance that put the rate of human reproduction on a level far below that free and unrestrained multiplication that characterizes lower forms. The first of these is legal,

[1]From the Department of Biology of the School of Hygiene and Public Health, Johns Hopkins University.

the second hedonistic. As one element of social organization mankind demands that the legal rite of marriage—this rite taking many diverse but basically equivalent forms amongst different peoples—shall normally and in the regular course be a prerequisite to human reproduction. To be sure this legal requirement is nowhere quite strictly enforced by the authorities nor adhered to by the subjects—even in these moral United States one baby out of every 25 to 30 odd born each year is illegitimate. But even so it is obvious that the institution of marriage is a major restriction upon the exuberance of multiplicative activity seen in lower forms.

The second, or hedonistic, restriction is what is commonly called "birth control". Mankind discovered long ago that the normal physiological mechanism of reproduction can be tampered with in a variety of ways, to the common end that while the probability of a particular copulation resulting eventually in progeny shall be greatly diminished, the pleasurable sensations associated with the initial act shall not be lessened at all, or at least not to anything like the same degree. Again there seems to be no doubt that this has become a restrictive factor on human reproduction of major importance, especially in modern urbanized and industrialized populations of high density.

II

It is the purpose of the present paper to examine, in a limited but so far as possible completely objective manner, the following question concerning the population of the United States in a particular year: What proportion of the women potentially capable physiologically of reproducing in 1930 actually reproduced in 1930? It should be clearly understood at the start that the answer can at best be only approximate, for reasons presently to be discussed, but even so it will perhaps be suggestive.

In the Birth Registration Area of the United States, which in 1930 included all the states except Texas and South Dakota, there were living 30,871,292[2] females between the ages of 15 and up to 50 years, of whom approximately 63[2] percent were married while 37 percent were either single, widowed, or divorced. But whether single, married, widowed, or divorced the vast majority of them were *physiologically* po-

[2]Computed from data given on pp. 183 and 237 of Abstract of the Fifteenth Census of the United States. *Washington* (Gov't. Printing Office) 1933. Pp. viii + 968.

tentially capable of reproduction. The average age of the menarche is not far from 15 years, and of the menopause not far from 48 years. Space is lacking to present here documentation, of which a large amount has been compiled to be elsewhere discussed, in support of this categorial statement, nor is it necessary for the purposes of the present discussion. What is more important is that in adopting the age limits just stated (15-49 inclusive) it is definitely certain that some females physiologically capable of reproduction have been excluded, because in 1930 there actually occurred 3026 births (living and still) to mothers under 15 years of age, and 160 births (living and still) to mothers aged 50 years and above. Let us start then with the initial general assumption (subject to later corrections) that in round numbers substantially all of these 30 odd million women between 15 and 49 years of age were *physiologically* potentially capable of reproducing in 1930. Theoretically then, these 30,871,292 women *might* conceivably have produced in 1930 at least 30,871,292 births, of at least some kind of product of conception, provided they had all set their minds and hearts and bodies to it (in the sort of way that flies or termite queens appear to do). For, be it noted, it is not physiologically impossible for a woman to reproduce once a year regularly over considerable stretches of years. Some do.

Actually in 1930 there were officially recorded as occurring in the same area, to mothers aged 15 to 49 years inclusive, 2,173,415 live births and 83,498 stillbirths, making a total of all births (living and still together) including illegitimate as well as legitimate births, of 2,256,913.[3] This does not represent quite all the reproductive activity of the year for a number of reasons. In the first place there occurred a considerable number of unrecorded abortions, natural and criminal. To attempt to estimate their number would be sheer guessing, so this item may as well be left for the moment as a recognized but unquantified error. A small offset against it is found in the fact that in the total recorded births multiple births (twins, triplets, etc.) are included, each of which required only one mother. In the second place, females under 15, and 50 years and over, in age together produced in the year 2970 live and 216 stillbirths, or 3186 births in total. But it has been deliberately decided to confine the discussion to the

[3]Computed from data given in Tables 4 and 13 of Birth, Stillbirth, and Infant Mortality Statistics for the Birth Registration Area of the United States 1930. *Washington* (Gov't. Printing Office) 1934. Pp. iv+400.

age limits 15 and 49 inclusive, because such a very small proportion of human females outside those limits are biologically capable of reproduction. Finally 27,573 live, and 2752 stillbirths, or a total of 30,325 were produced in 1930 by women whose ages failed to get recorded on the birth certificates. The majority of these women undoubtedly fell within the ages of 15 and 49 years inclusive, but precise information on the point is lacking.

Taking the figures as they stand, and realizing that they give only an approximation to the hidden truth, it appears that at least 2,287,196[4] births (living and still together) of 1930 were probably produced by mothers between 15 and 49 years of age inclusive. But these 2,287,196 births were produced by only about 7.4 percent of the 30,871,292 females physiologically potentially capable theoretically of reproducing in that year. Now it is undoubtedly true that some of these females were actually not physiologically capable of reproducing in that year by reason of true physiological sterility for which they were alone chargeable and in which their consorts played no part, or because of acute or chronic disabling illness, or because they were so malformed or otherwise so defective as literally to make mating a biological impossibility. Suppose 15 percent be deducted for this cause, which includes the supposition that 40 percent of all the women in the age group 45-49 had already passed the menopause. The percentage of births to total women then rises to 8.7 only. Also suppose it be assumed that the unrecorded abortions were four[5] times the recorded stillbirths, the stillbirths as now recorded being supposed in some states to include all products of conception delivered

[4]Derived by adding to the 2,256,913 births known to fall within the maternal age limits, the number of births, living and still together, born to mothers of unrecorded age reduced by 42 births, this being the number out of 30,325 expected to be produced by mothers under 15 and 50 and over in age, if the same ratio held in this group of births as holds for all births to mothers of known age.

[5]In 1930 the recorded stillbirths were 3.8 of all births (living and still). Pearl reported 1006 abortions in 6441 pregnancies, or 15.6 percent [Pearl, R. Statistical report on the fifth year's operations of the Bureau for Contraceptive Advice. *Fifth Rept. Bur. Cont. Adv. Baltimore,* pp. 5-17, 1933]. There is good reason to believe that in this series no significant number of abortions were unreported. Hence an *addition* of four times the already recorded stillbirths seems a reasonably liberal coverage. Green-Armytage [Sterile mating. *Lancet,* Vol. 231, pp. 426-427, 1936] states that in England "15 per cent of all pregnancies terminate in miscarriage", a figure in close agreement with that from the Baltimore experience just cited.

dead at whatever period of uterogestation, and do include *some* at every period except the very earliest, and add this figure to the births. The resulting percentage of actual to potential mothers, on the basis of the number of females reduced by 15 percent for reasons already mentioned, becomes only 10.0.

So, then, on the basis of liberal allowances for errors it appears that probably *not to exceed* 10 *percent of the women physiologically potentially capable of reproducing in* 1930 *actually reproduced.* Put in another way, this result means that the females 15 to 49 years of age, after the liberal deduction for disabilities already noted, produced in 1930 on the average only about 0.1 of a birth apiece (living and still together, with the allowance added for unrecorded abortions). Now the total duration of the physiologically possible reproductive life of the human female is a little over 30 years, as an average. Computations made in this laboratory from the data of Sanes[6] and Heyn[7] on the interval from menarche to menopause give the following means:

Sanes' data (Pittsburgh) ($N = 178$).....32.39 ± .27 years
Heyn's data (Reichenbach, Schlesien) ($N = 188$).....31.54 ± .22 years

Again to be liberal, take 33 years as a round figure definitely in excess of the evidence. Then it follows that if each of the women 15-49 years of age were to reproduce *annually* for 33 years at the average rate exhibited by the whole group in 1930, she would produce, as an average, in her whole reproductive life $33 \times 0.1 = 3.3$ births in total.

What impresses a biologist most strongly about this figure is its marked contrast to the conditions that prevail amongst organisms lower in the evolutionary scale than man. While it is not possible, because of considerations of space, to set forth here equivalent data for lower forms in any great detail, a few examples may be given. First consider the rat, by no means a particularly luxuriant reproducer —in fact quite the contrary as species go. The following figures are compiled from data published by King,[8] and in each case cover the whole life time of the mothers:

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[6]Sanes, K. I. Menstrual obstetrics: a study based on 4500 menstrual histories. *Amer. Jour. Obst.*, Vol. 73, pp. 93-112, 1916.

[7]Heyn, A. Ueber Menstruation, Haarfarbung und Libido und ihre gegenseitigen Beziehungen. *Zeitschr. f. Geburtsh. u. Gyn.*, Bd. 82, pp. 136-152, 1920.

[8]King, Helen Dean. Litter production and sex ratio in various strains of rats. *Anat. Rec.*, Vol. 27, pp. 337-366, 1924.

Strain	*Mean No. of Offspring Produced per Mother*	
Piebald (black-hooded) females ($N = 51$)	55.8	(p. 349)
Extracted Norway females ($N = 50$)	34.8	(p. 347)
Stock albino females ($N = 148$)	33.7	(p. 340)
Extracted albino females ($N = 57$)	28.0	(p. 346)
Norway females ($N = 88$)	21.2	(p. 343)

Evidently the poorest sort of reproductive performance in the rat is many times better than that exhibited by human females, even though allowance is made for the fact that no physiologically sterile rats enter into the averages.

Drosophila is a more prolific breeder than any mammal. How much so is evidenced by the following figures quoted from Alpatov[9] (p. 95) regarding total egg production throughout the life time of the fly:

Group	*Mean No. of Eggs Produced per Female*
Wild (reared at 19°C.; imaginal life at 25°) ($N = 37$)	941.1 ± 35.9 eggs
Wild (reared at room temp.; imaginal life at 30°) ($N = 26$)	713.8 ± 40.3 eggs
Vestigial (reared at room temp.; imaginal life at 30°) ($N = 35$)	432.6 ± 25.8 eggs
Wild (reared at 30°C.; imaginal life at 25°) ($N = 38$)	430.3 ± 33.4 eggs
Wild, underfed (reared at 30°C.; imaginal life at 25°) ($N = 27$)	315.4 ± 19.1 eggs

In round numbers these flies performed reproductively from one to three hundred times better than American females in 1930. Other examples similar in purport might be given, but it is not necessary.

It is evident that mankind, as a species, is not only a slow but also an indifferent reproducer. He facultatively alters the expression of the fundamental biological drive or urge to reproduction that is a part of his being, and that he shares in common with all other living things. Every year's birth statistics show examples of women who in a total reproductive span of about 30 years produce at least 24 to 29

[9]Alpatov, W. W. Egg production in *Drosophila melanogaster* and some factors that influence it. *Jour. Exper. Zool.*, Vol. 63, pp. 85-111, 1932.

births. This is not "indifferent" reproduction, considering the rigid limitations of the human reproductive mechanism with its long period of gestation, and considering in due biological perspective the length of man's life span. Such cases demonstrate the maximum physiological possibilities of the human reproductive machinery. But such women are extremely few in number. The vast majority are different. Individually, and racially, there appears to be, under *present* social and other environmental conditions, a slow but definite progressive trend towards less and less human *reproduction*, without any material lessening of human *sexual activity*. It seems probable that, while only about 10 percent of the women theoretically potentially capable of reproduction in 1930 actually reproduced, at least 70 to 75 percent of them were sexually active in greater or less degree in that year. Certainly the 63 percent married were, and so also were a lot of the single, widowed, and divorced, realistically considered. Here lies the fundamental biological contrast between man and the lower forms. When the latter are sexually active reproduction inevitably follows, generally speaking; whereas in the human species, for a variety of reasons, including contraception, partial or complete sterility, and sundry others, a vast amount of human sexual activity has no other result than hedonistic satisfaction.

It will perhaps be urged that the discussion so far is quite unfair on the ground that it should have been restricted to married women. Too much weight should not be attached to this point, however, since the whole discussion is professedly and of set purpose only *biological* in its viewpoint. Marriage is a legal and religious institution, not fundamentally a biological one. By and large there is no biological reason why the unmarried women in the total group under discussion, should not be married, mating and reproducing. In other words this restriction on their reproduction is not biological in character. Nor, for that matter, and in the same sense, is contraception. There is no law, natural or political, that compels people to practice birth control if they do not want to.

In short all that has been attempted in this section is an objectively realistic examination of the performance of a substantial sample of human females as reproducing animals, in comparison with the performance of other animals lower in the evolutionary scale. The examination covers one year only; but, except for slow secular trends, any other recent year would show essentially the same result.

III

So far the discussion has been of the females biologically potentially capable of reproduction as a whole group. It is desirable now to examine separately into certain sub-classes, of biological and sociological interest, and their mutual interrelations. To do this has entailed some complications and difficulties because in the first place, in each instance Texas and South Dakota had to be deleted from the total figures, and also because the color (race) classifications of the data used by the Census Bureau in compiling birth figures in 1930 were not completely consilient with those used in enumerating the population. The discussion which follows relates to *live births* only.

Tables 1 and 2 give respectively the absolute numbers of (a) live births in the Birth Registration Area in 1930 to women aged 15 to 49 years inclusive, and (b) the total number of women falling within the same age limits, enumerated in the same area in the census of 1930, the totals in both cases being divided into sub-classes as indicated by the marginal rubrics.

TABLE 1

Number of Live Births in U. S. Birth Registration Area to Women Aged 15-19 Years Inclusive in 1930 by Age, Color, and White Nativity

AGE OF MOTHER	NATIVE WHITES	FOREIGN-BORN WHITES	WHITES OF UNRECORDED NATIVITY	COLORED	TOTALS
15-19	203,069	8,658	202	55,149	267,078
20-24	520,444	49,954	371	79,084	649,853
25-29	425,553	71,619	259	52,106	549,537
30-34	282,997	62,320	197	32,932	378,446
35-39	169,753	48,336	131	22,671	240,891
40-44	54,672	18,344	71	6,898	79,985
45-49	4,727	1,832	5	1,061	7,625
Totals	1,661,215	261,063	1,236	249,901	2,173,415

TABLE 2

Number of Women Recorded as Aged 15-49 Years Inclusive in the U. S. Birth Registration Area in 1930, by Age, Color, and White Nativity

AGE	NATIVE WHITES	FOREIGN-BORN WHITES	NEGRO	OTHER RACES	TOTALS
15-19	4,628,565	166,788	605,746	53,434	5,454,533
20-24	4,222,402	340,409	600,490	48,244	5,211,545
25-29	3,616,428	502,264	528,438	46,170	4,693,300
30-34	3,275,763	592,085	414,652	38,726	4,321,226
35-39	3,107,440	733,847	427,632	35,647	4,304,566
40-44	2,596,743	725,116	323,860	25,498	3,671,217
45-49	2,232,404	675,745	286,305	20,451	3,214,905
Totals	23,679,745	3,736,254	3,187,123	268,170	30,871,292

It will be noted at once that the birth statistics do not separate Negroes as such, but throw them into a general category "Colored". The population figures separate Negroes as a class, and have another rubric "Other Races", which includes Mexicans, Indians, Chinese, Japanese, Filipinos, Hindus, Koreans, Hawaiians, Malays, Siamese, and Samoans.

Tables 3 and 4 are percentage tables derived from the absolute statistics of the two preceding tables. The method of derivation of these tables will be made clear by specific examples. The upper left-hand corner cell of Table 3 contains the percentage 9.34. This means that 9.34 percent of all the fully recorded live births of 1930 to women aged 15-49 (there having been all told 2,173,415 such births) were to native white women aged 15-19 inclusive (there having been 203,069 births to such women). The percentage of 14.99 in the upper left-hand corner cell of Table 4 means that of the 30,871,292 women aged 15-49 inclusive just under 15 percent were native whites aged 15-19 inclusive.

TABLE 3

Percentage Distribution of Live Births in 1930, Born to Mothers Aged 15-49 Years Inclusive, in the U. S. Birth Registration Area (Percentages Less than 0.01 Omitted)

AGE OF MOTHERS	NATIVE WHITE MOTHERS	FOREIGN-BORN WHITE MOTHERS	WHITE MOTHERS OF UNRECORDED NATIVITY	COLORED MOTHERS	TOTALS
15-19	9.34	0.40	0.01	2.54	12.29
20-24	23.95	2.30	.02	3.64	29.90
25-29	19.58	3.30	.01	2.40	25.29
30-34	13.02	2.87	.01	1.52	17.41
35-39	7.81	2.22	.01	1.04	11.08
40-44	2.52	0.84	..	0.32	3.68
45-49	0.22	0.08	..	0.05	0.35
Totals	76.43	12.01	0.06	11.50	100.00

TABLE 4

Percentage Distribution of Females, Aged 15-49 Years Inclusive, in the United States (Less Texas and South Dakota) in 1930

AGE	NATIVE WHITES	FOREIGN-BORN WHITES	NEGRO	OTHER RACES	TOTALS
15-19	14.99	0.54	1.96	0.17	17.67
20-24	13.68	1.10	1.95	.16	16.88
25-29	11.71	1.63	1.71	.15	15.20
30-34	10.61	1.92	1.34	.13	14.00
35-39	10.07	2.38	1.39	.12	13.95
40-44	8.41	2.35	1.05	.08	11.89
45-49	7.23	2.19	0.93	.07	10.41
Totals	76.70	12.10	10.33	0.87	100.00

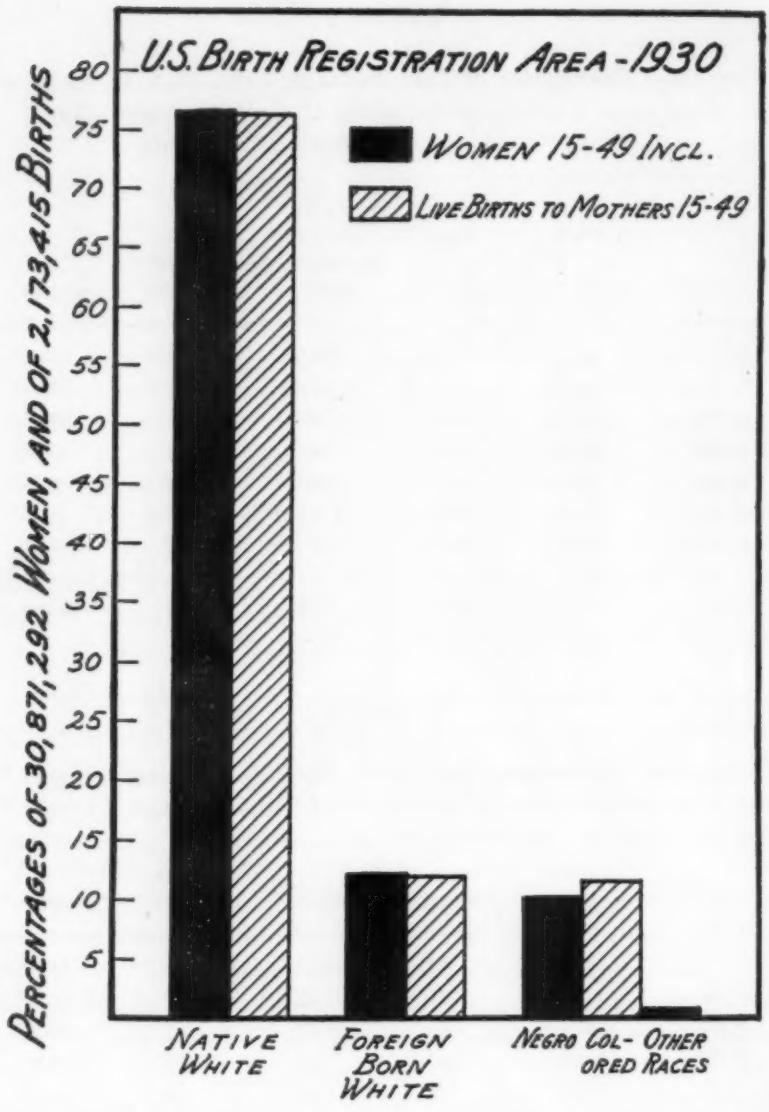

FIGURE 1. THE RELATIONSHIP BETWEEN WOMEN AGED 15-49 YEARS INCLUSIVE (SOLID BARS) AND LIVE BIRTHS TO WOMAN WITHIN THE SAME AGE LIMITS (CROSS-HATCHED BARS) IN 1930, BY NATIVITY AND RACE.

An examination of Tables 3 and 4, and of their relations to each other, reveals a number of points of interest. The relationships of the marginal totals of Tables 3 and 4 are shown graphically in Figures 1 and 2.

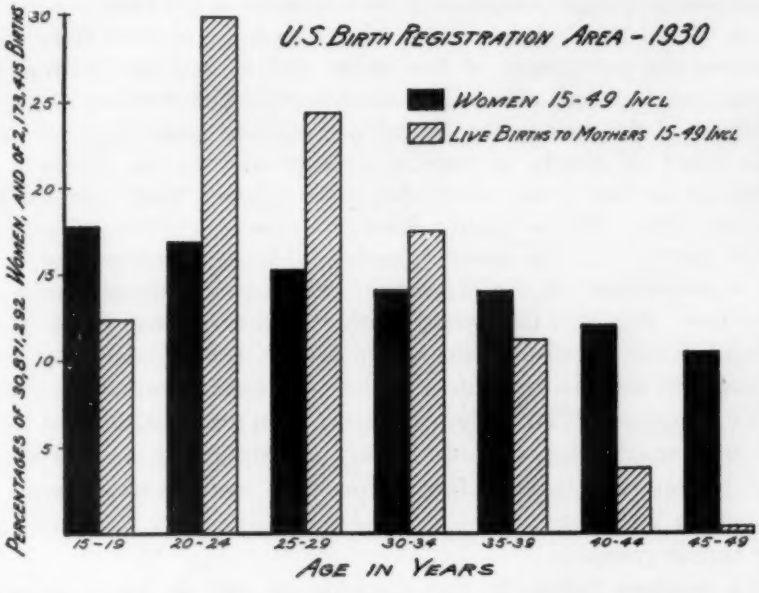

Figure 2. The Relationship between Women Aged 15-49 Years Inclusive (Solid Bars) and Live Births to Women Within the Same Age Limits, (Cross-Hatched Bars) in 1930, by Age.

From Tables 3 and 4 and Figures 1 and 2 the following points may be noted:

1. More than three-quarters of all the births were to native white mothers. Also more than three-quarters of all the women who potentially might have been mothers were native whites. Of the remaining births 11.5 percent were to colored mothers, of whom probably over 92 percent were Negroes. Of the potential mothers (all women between 15 and 49 years of age inclusive) 10.33 percent were Negroes and less than one percent of "Other Races", most of whose live births, insofar as they had them, were probably recorded in the "colored" birth category. The sum of the percentages of potential mothers in

these two race (color) categories (10.33 + 0.87) is 11.20, a figure again in very close agreement with the percentage of colored live births (11.50). The residue of births and potential mothers were in the foreign-born white class. The potential mothers in this class constituted 12.10 percent of all potential mothers, and the actual mothers in the class produced 12.01 percent of all the live births of the year.

2. Taken as a whole picture the closeness of agreement in 1930 between the percentages of live births and of potential mothers by nativity-race (color) classes is extraordinary. The numbers of actual mothers of the year in the several nativity-race (color) classes were just about as closely as possible proportionate to the numbers of potential mothers from whom they were drawn. *Each nativity-race (color) class, whether native white, foreign-born white, Negro, or other was as a whole group reproducing in 1930 proportionately to its representation in the population, with a near approach to exact precision.* Any idea that foreign-born women as a group in the whole United States population are at the present time out-breeding their native-born sisters—a notion of rather widespread journalistic prevalence at least—finds no vestige of support from the actual facts of 1930. As will presently appear, however, this equality in relative *total* breeding performance between foreign-born and native white women as groups, is a consequence primarily of the distorted age distribution of the former group.

3. Mothers falling in ages between 20 and 34 years inclusive produced 72.6 percent of all the live births in 1930; and native white mothers within the same age limits produced over half (56.55 per cent) of all the live births. But of all the potential mothers only 46.08 percent fell within the age limits 20-34; while the native white potential mothers within the same age limits constituted but 36 percent of all potential mothers. It is thus evident that the women between 20 and 34 years of age were contributing to the live births of the year considerably in excess of their proportionate representation in the population. This excess is greatest in the age period 20-24 years. It is in that quinquennium that American women, taken as a whole group, exhibit the highest tide of their reproductive activities. It is in these 15 years of life between 20 and 34 that the largest proportion of women permit themselves to be transposed from the category of potential to actual mothers.

4. In no other age class outside of the limits 20 and 34, either earlier or later, are the percentages of live births as great as the corresponding percentages of potential mothers. In other words, in the outlying age classes the proportionate numbers of actual mothers are in defect, as compared with those of potential mothers. Physiology and prudence are undoubtedly the major elements in producing this defect.

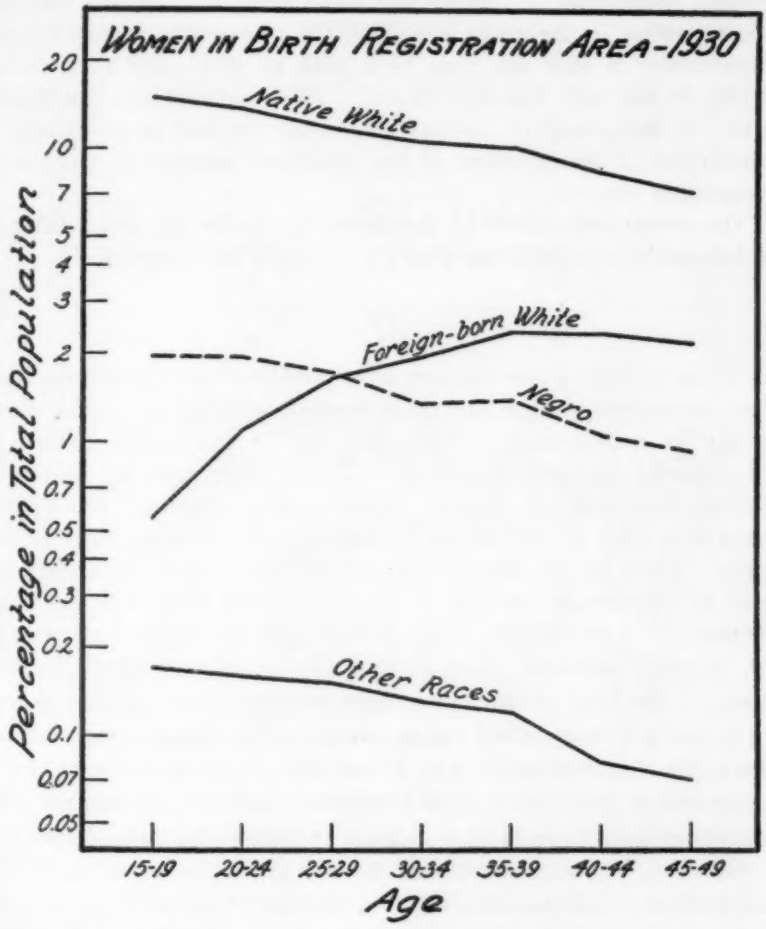

FIGURE 3. AGE DISTRIBUTIONS OF WOMEN AGED 15-49 YEARS INCLUSIVE IN THE U. S. BIRTH REGISTRATION AREA, 1930.

5. The age distributions of the several nativity-race (color) groups are evidently quite different. Among the native whites the highest proportion of women aged 15-49 years inclusive is found in the youngest age class (15-19 years). In the succeeding age classes the numbers of native white women become progressively smaller. The age distributions of the Negro and "Other Races" women are, with some irregularities, similar in form to that of the native white women. But the age distribution of the foreign-born white women is widely different from that of any of the other groups. Starting with the lowest number in the 15-19 age class, the proportionate numbers rise progressively in each age class to a peak at 35-39, and then decline slightly in the next two age classes. This distorted age distribution curve for the group of foreign-born white women is presumably a consequence of the operation of the restrictive features of the present immigration law.

The extent and nature of the distortion of the age distribution of the foreign-born women are shown graphically in Figure 3.

IV

Tables 3 and 4 are numerical expressions of contour surfaces. They respectively define the three-dimensional relative spatial distributions by nativity-race (color) and age of live births (Table 3) and potential mothers (Table 4). These tables may be looked on from another point of view as expressions of empirical probabilities (chances in 100) of the concurrent happening of the marginally defined events. Thus, *on the basis of the experience of 1930 only,* there are about 24 chances in 100 that a just born living baby will have been produced by a native-born white mother aged somewhere between 20 and 24 years inclusive. And so similarly for other cells of Table 3. Again, on the basis of the 1930 census findings, there are 1.71 chances in 100 that a woman picked out at random within the geographical confines of the Registration Area as it then existed, about whom nothing is known except that she is aged somewhere between 15 and 49 years inclusive, will turn out to be a Negress in the 25 and 29 year age class.

Each of the contour surfaces defined in Tables 3 and 4 is self-contained and independent—that is, relates exclusively to its own defined universe of discourse. They have not so far been numerically inter-related to each other. What is now wanted to complete the

picture is a *nativity-race(color)—age contour surface of fertility* (or expressed reproductivity). This is provided in Table 5.

TABLE 5

Nativity—Race (color)—Age Contour Surface of Female Fertility Based Upon the Experience of 1930 in the U. S. Birth Registration Area as Then Defined

AGE OF MOTHER	NATIVE WHITES	FOREIGN-BORN WHITES	COLORED	WEIGHTED MEANS
15-19	4.39	5.19	8.37	4.89
20-24	12.33	14.67	12.19	12.46
25-29	11.77	14.26	9.07	11.70
30-34	8.64	10.53	7.26	8.75
35-39	5.46	6.59	4.89	5.59
40-44	2.11	2.53	1.97	2.18
45-49	0.21	0.27	0.35	0.24
Weighted Means	7.02	6.99	7.23	7.04

The manner of construction of Table 5 and its meaning will be made clear by the following description. Using a single cell, to make the description precise and concrete, it will be seen by reference to Tables 1 and 2 that in 1930 in the Registration Area there were 71,619 live births to foreign-born white mothers age 25-29 years inclusive, and that there were in the same area and year 502,264 foreign-born white women in total. $71{,}619 \times 100/502{,}264 = 14.26$ percent, the figure entered in the corresponding cell of Table 5. This means that in 1930—neglecting the slight error consequent upon the occurrence of multiple births—14.26 percent of all the foreign-born women (single, married, widowed, or divorced) living in the Birth Registration Area and aged 25-29 years inclusive produced a live birth within the year, and that 85.74 percent of the so-defined women did not produce a live birth in that year. In other words, and again excepting the error involved in multiple births, the figures of Table 5 show the chances in 100, *on the basis of the experience of 1930 only,* that a woman of specified race (color) and age status will pass in a particular year from the category of potential mother over into the category of actual mother of a living child.

There are two minor technical points that must be mentioned in connection with the construction of Table 5. Owing to the different practice, already discussed earlier, of the census and registration officials relative to race (color) classification, colored live births in Table 5 are expressed as percentages of Negro and "Other Races" women combined. Thus the 7.26 percent of Table 5, expressing percentage of colored live births to women 30-34 years of age is derived from 3,293,200/(414,652 + 38,726). It seems probable that the net residual error embodied in Table 5 from this source cannot be great enough to be of any practical importance. There are two reasons why this seems likely. In the first place it is certain that the great majority of the colored births of Table 1 are Negro births, because the aggregate number of "Other Races" women in the portion of the population under discussion is only about 8 percent of the number of Negro women. In the second place it seems probable that a considerable, though unascertainable, fraction of the births to "Other Races" mothers were in fact entered on the birth certificates as "colored".

The other point concerns the small group of "Whites of unrecorded nativity" that appears in Table 1, but not in Table 2. This group affected in no way the computation of Table 5 except in respect of the right hand marginal mean percentages. There the births in the unrecorded nativity class were subtracted from the marginal totals. Thus for age 15-19, 4.89 = (267,078 — 202) × 100/5,454,533. This is not an exact procedure, because mothers of unrecorded nativity are still concealed amongst the women, even though their births have been deleted. However, the numbers involved are so small relatively that the resulting error is insignificant.

The significance of the figures of Table 5 having been made clear, the results worthy of note may be discussed. These are:

1. Taking the women of all ages from 15 to 49 inclusive as whole groups it appears that there is no substantial difference in weighted mean group reproductivity rate between native whites, foreign-born whites, and colored groups. On the average about 7 percent of the potential mothers in each of these groups became actual mothers of live babies in 1930. But while the differences between the nativity-race (color) groups as wholes in mean relative fertility are absolutely too small to be of any practical importance, some of them are statistically significant in comparison with their probable errors. This is not the case for the native and foreign-born white groups, where the

difference is 7.02 — 6.99 = .03 ± .01, which can hardly be regarded as certainly statistically significant. Taking the figures at their face value, however, the weighted mean relative fertility of the colored group as a whole is significantly higher than that of either the native or foreign-born group, the differences being respectively 7.23 — 7.02 = .21 ± .01 and 7.23 — 6.99 = .24 ± .01. Both these differences are more than 20 times their probable errors. But one feels that the point is really academic, because it still remains the fact that these differences in means are too small to be of any practical importance, and furthermore the figure for the colored suffers from an intrinsic disability that cannot be precisely resolved, as has been discussed above. The really impressive fact appears to be that these nativity-race (color) whole group weighted mean relative fertilities turn out so nearly alike as they do.

2. An interesting statistical paradox is presented by the fact that while the age specific relative live birth fertilities of the foreign-born whites are for every single age group from 15-19 through 45-49 *higher* when taken separately than those for native whites, the mean relative fertility of the foreign whites as a whole group is slightly lower than that of the native whites as a whole group. The explanation is found, of course, in the distorted age distribution of the foreign-born white potential mothers, and, in consequence, of the foreign-born actual mothers.

3. A similar though less marked paradox is found in the comparison of the total group and age specific relative fertilities of colored and both sorts of white groups. In only two age periods (15-19 and 45-49) are the age specific relative fertilities of the colored greater than those of the whites. Yet as a whole group the colored exhibit a significantly higher weighted mean relative fertility than that of either white group. This again is in part a consequence of peculiarities in age distribution.

4. In 1930 the conversion of potential capacity for motherhood into actual motherhood reached its highest peak among the foreign-born women aged 20-24 years. For all three nativity-race (color) groups it is in this age period that relative fertility is highest. The much higher relative fertility of the colored group in the youngest age period (15-19) is noteworthy. The excess reproductive performance of the colored in this age period over both white groups is, of course, certainly significant statistically. The difference between colored and

foreign-born whites is $8.37 - 5.19 = 3.18 \pm .44$ or more than 7 times its probable error. The difference between colored and native white in this group is still larger, and its probable error smaller.

DISCUSSION AND SUMMARY

In this paper an attempt is made to determine as accurately as possible the proportion of women in the United States population (excluding Texas and South Dakota for lack of data) who, being biologically potentially capable of becoming mothers in 1930, actually did in fact achieve motherhood during that year. With the most painstaking effort the result can be only approximate rather than exact, because some of the necessary figures are either not recorded at all, or are compiled in such a way that they cannot be separately disentangled. But the approximation that can be reached by applying reasonable corrections at the difficult points is probably fairly close to the unknowable exact truth.

The net upshot is that in 1930 probably not to exceed 10 percent of the potential mothers, in a biological sense, actually became mothers. Any other recent year would undoubtedly lead to the same result, if data were available for the computation.

The derivative consequence of this figure in terms of total reproductivity is compared with data on the reproductive performance of some animals lower in the evolutionary series than man. The result is to emphasize in numerical terms what is generally evident—that man as a species is an indifferent reproducer as compared with other species. This is in part due to his facultative alteration, for fundamentally hedonistic reasons, of the normal biological operation of his reproductive mechanism. Under present environmental conditions, and especially under the present social and economic environment, there seems to be evidence of a slow progressive trend towards more of such facultative interference. If such a trend were to persist indefinitely man as a species would eventually disappear from the face of the earth for want of reproductivity. Some recent writers are bold enough to make just such a prediction for the not distant future. It seems unlikely.

Analysing further the specific problem of this paper, it is shown that, taken as whole groups, the native white, foreign-born white, and colored (Negro and Other Races) women were each reproducing in 1930 in almost precisely the exact proportion of their representation

as potential mothers in the whole population. Since in round figures three-quarters of the births of 1930 were to native white mothers, and three-quarters of all the potential mothers in that year were native white, it may be not unreasonably inferred that America is reproductively predominantly still of and for the Americans. If it be granted, as again is not unreasonable, that the Negroes of this country are in the main Americans too, the case is still stronger. Put the other way about foreign-born white women in 1930 were doing only about 12 percent of the effective (live birth) reproducing in this country. The foreign-born potential (and actual) mothers in 1930 exhibited a greatly distorted age distribution, resulting primarily from the operation of the present immigration law. The age specific relative fertilities of the foreign-born white women are higher than those of the native whites for every age period from 15-19 years through 45-49 years. With the same age distribution as the native whites, the foreign-born group would exhibit a significant group fertility differential, which they now do not show as a whole group average because their age distribution is so distorted.

The bulk of reproducing is done by women between the ages of 20 and 34 years. In 1930 46 percent of all the potential mothers fell within this 15 year age range, but the actual mothers of the year within the same age range produced over 72 percent of all the live births. It is in the age period 20-24 years that relative fertility reaches its highest peak in all the nativity-race (color) groups.

We have seen that the native white, foreign-born white, and colored women in this country potentially capable of reproduction were reproducing in 1930 as whole groups in almost exact proportion to their representation in the population. Also their weighted mean relative fertilities, on a live birth basis, were, for the groups as wholes, nearly equal. These facts present a pretty set of logical difficulties when they are considered relative to the practice of birth control in the population. Consider first native white and colored groups. The evidence from general experience and observation, as well as from specific investigations,[10] is that Negroes attempt contraception much less frequently, and are less effective about it when they do, than whites in the American population and especially native whites. Yet, as Table 5 shows, as a group, with a not seriously different age distribu-

[10]Pearl, R. Third progress report on a study of family limitation. *Milbank Memorial Fund Quarterly*, Vol. 14, pp. 258-284, 1936.

tion, the colored women exhibit a weighted mean live birth fertility that is only a little higher than that of the native whites. Furthermore, rather precise and trustworthy evidence[11] indicates that in the complete absence of contraceptive effort and of diseases of reproductive organs, the net natural fertilities of whites and Negroes are substantially identical age for age. Now actually the colored group in the present material, between the ages of 20 and 44 inclusive, shows lower age specific live birth fertilities than the native whites (Table 5) in spite of the fact that birth control is practiced far less.[12] Perhaps the greater prevalence of venereal disease in the Negro population—an undoubted fact—is the explanation. But also it is possible that in numerically large population groups, as these under discussion are, birth control in either whites or colored is not playing as important a rôle in determining expressed fertilities as it is currently supposed to be. Perhaps fundamental biological factors are more important. Relative frequency of marriage will not greatly help here in endeavoring to solve the riddle, because the Negroes are not so much hampered in reproductive activities by pre- or extra- marital taboos as the native whites. The problem is, in the present status of available data, insoluble at this time. But the logical dilemma is clear. Two large groups of women, both potentially capable of reproduction, and both having substantially the same sort of age distribution, exhibit as whole groups about the same mean relative fertility, yet one practices birth control very much more, and more effectively, than the other.

Consider next the native and foreign white groups relative to contraception. It has already been pointed out that the chief reason why they show, as whole groups, about the same mean relative fertility is found in the distorted age distribution of the foreign-born women. So that need not be discussed further. Presumably, on currently prevalent views among population students, all or nearly all the superiority of the foreign-born in age specific fertilities over the native shown in Table 5 is to be explained by less contraception among the foreign-born women. Undoubtedly there *is* less, and what there is is probably less effective. Accepting this temporarily as a fact, for the sake of the argument, the difference between the two sets of age specific fertilities should then theoretically be a rough measure of the

[11]Pearl, R. Fertility and contraception in urban whites and Negroes. *Science*, Vol. 83, pp. 503-506, 1936.

[12]Not all the differences are statistically significant, but some are.

net effectiveness of the more prevalent and better contraception of the native whites. Let us examine the figures. To facilitate the examination and comparison we define:

Percentage A = [Native white age specific fertility (Table 5)] × 100/Foreign-born white age specific fertility (Table 5)

Percentage B[13] = [Age specific median pregnancy rate of white women practising contraception most intelligently] × 100/Age specific median pregnancy rate of white non-contraceptors

Percentage C[13] = [Age specific median pregnancy rate of white women practising contraception most intelligently] × 100/Age specific median pregnancy rate of white women practising contraception least intelligently

We then have:

Age Class	*15-19*	*20-24*	*25-29*	*30-34*	*35-39*	*40-44*	*45-49*
Percentage A	84.6	84.0	82.5	82.0	82.9	83.4	77.8
Percentage B	11.3	35.7	51.7	72.9	88.7	103.2	
Percentage C	19.5	49.8	69.3	92.6	125.4	82.4	

It seems at once evident from these figures that the differences in age specific fertilities between the native and foreign-born whites of Table 5 *supra* are not at all of the sort known to be associated with differences in amount and effectiveness of contraceptive practice in more precisely controlled and thoroughly studied samples of American women. The relative differences in fertility between the native and foreign-born white groups of the present paper are substantially constant for all age periods during the reproductive period. But where the differences in expressed fertility are known to be associated primarily with differences in contraceptive practice they are greatest at the lowest ages, and rise steadily with advancing age. This suggests that the native-foreign-born fertility differentials of the present material are probably due primarily to some *other* factor (genetic or environmental or a combination of both) than contraception. This factor may conceivably be racial. I doubt that it is wholly so, though it may be partly. Again we are confronted with a puzzle that cannot be solved with the information at present available.

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[13]Taken from table on p. 281 of Pearl, *loc. cit.*, in footnote 10 *supra*.

The moral that impresses itself upon the writer's mind from all this is that we are still a long way from being able to form a really sound judgment as to the precise nature and degree of the effect of contraceptive practices upon the overt or expressed reproductivity of large population aggregates.

A COMPARATIVE STUDY OF FINGER PRINTS AND TOE PRINTS

BY MARSHALL T. NEWMAN

Department of Anthropology, University of Chicago

INTRODUCTION

WORKERS in Dermatoglyphics in the past forty years have given a great deal of attention to the study of epidermal ridge configurations of the fingers and palms. Sole patterns, while not so extensively investigated, have not been neglected. But toe patterns have hardly been studied, the only significant analyses being those of Hasebe (10, pp. 79-82) who investigated the toes of 100 Japanese males, and Takeya (16) who conducted a similar study on 1000 North Chinese males. At an earlier date Fére (7, 8) undertook several pieces of research on toe patterns, but his method of pattern classification is not reconcilable with modern dermatoglyphic technique. Except for these several contributions, the field of toe patterns remains virtually untouched.

Probably the main reason toe patterns have received so little attention is the difficulty in securing adequate prints. Complete and clearly decipherable prints of all but the great toe are very difficult to procure, and the process is necessarily time-consuming.

In view of this neglect of toe patterns it seemed worth while to seek answers to the following problems:

(1) What are the qualitative differences, if any, between finger patterns and toe patterns?

(2) Are there quantitative differences in the expression of patterns on fingers and toes?

(3) Has the large number of reversed patterns on the 2nd (index) finger any counterpart in toes?

(4) Does a comparison of finger prints and toe prints bear out the findings of workers in other fields that the axis of the foot differs from that of the hand?

(5) What is the degree of correlation in the pattern expression of fingers and toes in the same individual?

(6) Is there any demonstrable bilateral asymmetry in the pattern expression in toes comparable to that present in fingers?

(7) Do toe patterns show racial differences comparable to those shown in finger, palm and sole patterns?

I wish to express my appreciation to my father, Prof. H. H. Newman, who supervised the study, and to Dr. Harold Cummins for his invaluable suggestions and criticism. For the application of statistical methods to the work I am indebted to Dr. Sewell Wright and Dr. H. H. Stranskov. I also wish to thank Prof. Arthur K. Loomis, Mr. Leslie Irwin, and Mr. Galvin Walkel of the University High School, Chicago, for making it possible for me to secure the prints.

MATERIALS AND METHODS

1. *Selection of Cases*

In order to deal adequately with the problems mentioned, it was necessary to select the subjects rather carefully.

In the first place, the series was limited to males, since Newman (12, pp. 308-12) and others find slight sexual differences in the finger prints of males and females.

Secondly, since a well defined bimanual asymmetry in pattern expression has been discovered for finger prints, and since right-handers and left-handers are somewhat reversed in this respect, it seemed advisable to confine the present study to right-handers. However since right- and left-handedness are not very definite types of habit in that there are various degrees of both manual habits, including ambidexters and weak right- and left-handers, it is difficult to diagnose the border-line cases. In the present group we have escaped this difficulty by using only full right-handers, discarding all questionable cases. Newman has gone extensively into the diagnosis of handedness by behavior tests (12, pp. 281-87), and profiting by his experience, only the most reliable criteria are used here. These are writing, throwing, use of a one-handed instrument, and kicking(*viz.*, foot used in kicking a ball). It is possible that several border-line cases crept into our series, but their presence probably makes no appreciable difference in the total results.

Thirdly, since a racial comparison was one of the proposed lines of investigation, it seemed advisable to keep the series as racially homogeneous as possible. Hence an effort was made to select subjects from predominantly North and Central European stocks.

The series, then, consists of both finger and toe prints of 100 right-handed European-American males. That the size of this series is adequate is born out by the fact that when the data are checked against those from other series, all the major findings are comparable. The slight divergences that do occur can be attributed to racial differences and to differences in the selection of cases. For example, neither Hasebe or Takeya eliminated left-handers nor border-line cases from their series. For purposes of comparison it is to be regretted that Takeya did not finger print as well as toe print his subjects, and that although Hasebe did record finger prints, his presentation of these data does not permit individual analysis.

2. *Method of Printing*

The printing technique used here is an adaptation of Prof. Mathews' photo-paper method (5, pp. 422-23). Small squares of photographic printing paper are pressed against the balls of the individual toes after the toes have been moistened with a printing solution. In printing it is helpful to back the printing paper squares with a square piece of one-sixth inch gasket rubber. These prints, of course, must be fixed in a hypo solution later. After some practice it was found that the complete set of finger and toe prints of a subject could be taken in ten or fifteen minutes.

3. *Pattern Classification*

If the patterns on fingers and toes naturally fell into clear-cut categories, one of the dermatoglyphists' chief problems would be removed. Such, however, is not the case. Pattern types blend into one another in such a confusing manner that it is almost impossible to classify border-line types with certainty. Hence rather arbitrary methods of classification must be adopted. For this reason an account of these methods seems to be pertinent.

Most investigators have used the threefold classification: whorls, loops, and arches. Usually loops are divided into radial and ulnar types for fingers, and tibial and fibular types for toes. Since most tabulations

have been made on the basis of these three or four categories mentioned, it is necessary to adopt these classifications for comparative purposes. Here in order to differentiate between the specific and the inclusive meanings of the terms, whorl, loop, and arch, quotation marks are used to indicate when the terms are used in the inclusive sense.

Certain additional items of pattern description are useful in dealing with the biological aspects of pattern expression. Not only must the classification of border-line types be subjected to a definite set of rules, however arbitrary, but the already classified patterns must be more specifically described than simply as "whorls", radial or ulnar "loops", or "arches". The direction of pattern twist, for example, is important in determining the distribution of radial or reversed patterns on fingers as well as toes.

The pattern classification employed here is merely an effort to include the many varieties of patterns in 22 convenient classes. These classes are ranged under the three divisions: "whorls", "loops", and "arches".

No phylogenetic development is implied in this classification, and the types listed here are considered transitional to one another only in a descriptive sense. As Cummins (4, pp. 1-4, 14-22) points out, the occasional presence of "accidental" patterns which do not fit into any seriated scheme yet devised, makes it difficult to put much faith in an evolutionary concept of pattern morphology which insists on deriving one pattern type from another. Cummins would rather explain the formation of different patterns on the basis of ontogenetic factors. We shall again refer to these important views. Suffice it to say now that Cummins' statements come as a word of caution to the unwary. In this light the classification set down in Table 4 must be considered as a purely descriptive one.

The value of this classification lies in its usefulness in comparing the data of this study with those of other studies. Since the results obtained here and the results of others are well in agreement, it is assumed that this system of classification is valid for comparative purposes at least.

4. *Statistical Methods*

Although much of the material in this paper is not adaptable to statistical technique, some of the data are ideal for such treatment. In dealing with intra-individual correlations in pattern expression

between fingers and toes, statistical analysis is indispensable. The formulae used here are standard, and hence will be mentioned only in passing.

In determining whether intra-individual correspondences in pattern expression between homologous digits of the hands and feet are statistically significant, the formula for the Standard Error of Percentages is used.

In dealing with intra-individual correspondences in pattern expression between total fingers and total toes, the method using the Coefficient of Correlation and the Standard Error of the Coefficient of Correlation is employed. All the foregoing methods were adopted only after consultation with competent statisticians.

QUALITATIVE DIFFERENCES IN PATTERN EXPRESSION IN FINGERS AND TOES

1. *Pattern Shape*

The causal relationship between volar pad and ridge pattern is ably discussed by Cummins (4, p. 22) who states: "The integument of the volar surfaces possesses the inherent capacity to form ridges, but the disposition of these ridges is controlled by the growth of the part." From this it becomes evident that in order directly to correlate pad form and pattern form, it is necessary to examine them at the time of the development of the ridges themselves. This, according to Pinkus (15), is sometime after the 18th intrauterine week. At this time the volar pads are becoming less prominent, and it is important to note that foot differentiation lags behind that of the hand. These data, best presented in two papers by Cummins (2, 3) are most pertinent in a study of pattern shape.

In this connection it is interesting to note the extreme foreshortening of the toe pads as compared with the finger pads. There is, moreover, not only a foreshortening but a broadening of the toe pads.

If, then, sometime after the 18th intrauterine week, each toe is shortened along the longitudinal axis and broadened along the transverse axis, the epidermal patterns determined at that time must conform to this shape. That they do is strongly brought out by the flatness of toe patterns as compared to finger patterns. While finger patterns are usually of the high, well-vaulted type, toe patterns are commonly

low and frequently so spread out laterally as to extend to the sides of the toe and to fade out on the ridgeless skin.

The great toe pad seems to be the least foreshortened. Its patterns, however, are generally small in comparison with the total ridged surfaces, and are not highly vaulted. These strikingly small patterns are probably to be explained by the fact that at the time the epidermal ridges are laid down, the large toe is but slightly larger than the others, and its pad scarcely larger at all. The subsequent increase in relative size of the great toe over the others does not seem to involve a proportionate increase in its pattern surface.

2. Reduction in Pattern Expression in Toes as Compared to Fingers

As was mentioned before, the differentiation of the foot lags behind that of the hand. This condition may well be correlated with the smaller number of highly expressed patterns on toes as compared

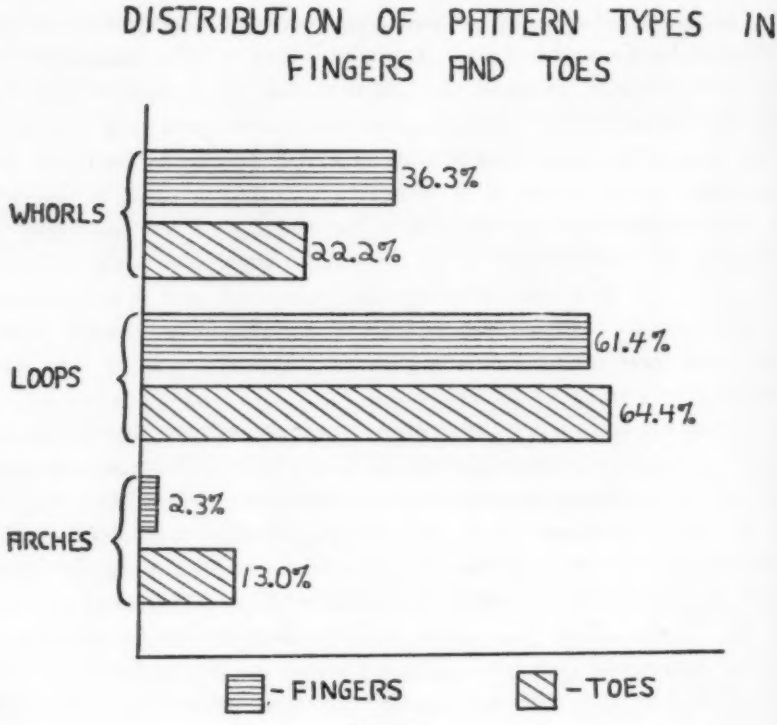

FIGURE 1

to fingers, for it is quite probable that the relatively late and slow embryonic development of the foot tends to suppress pattern expression. An additional factor making for differences in pattern expression may be associated with unlike contours of finger and toe pads, independent of the time relations.

That a suppression or reduction in pattern expression is present in toes as compared with fingers is demonstrated graphically in Fig. 1. Here "whorls", the most highly expressed patterns, are considerably less frequent on toes. "Loops", a more reduced type of pattern than "whorls", are somewhat more numerous on toes, while "arches", the most reduced patterns of all, are far more numerous on toes.

The amount of pattern reduction, while indicated in the distribution of pattern types, might seem to call for determination by quantitative means, such as the Bonnevie ridge count (1, pp. 46-54). Regrettably, however, the triradii are so often lost on the ridgeless skin, or are so far on the sides of the toe as not to be included in the print, that ridge counting is not feasible. Nevertheless the quantitative reduction of toe patterns is obvious from superficial examination of the greatly flattened patterns, and from the low ridge count in those prints that are complete. So it seems safe to say that toe patterns are markedly reduced both qualitatively and quantitatively as compared with finger patterns.

3. *Pattern Distribution Among Individual Fingers and Toes*

This absolute reduction in expression of toe patterns as compared with finger patterns is not evenly distributed over the five toes. A comparison with pattern distributions on individual fingers yields the following striking features which are shown graphically in Fig. 2.

(1) Quite striking is the high percentage of "whorls" on the 3rd toe, while the 3rd finger shows a preponderance of "loops", and a less than average allotment of "whorls". The high incidence of "whorls" on the 4th finger, however, parallels the situation in the 3rd toe.

(2) Slightly less striking is the large number of "arches" on the 5th toe. Here no "whorls" are present, although they run as high as 25 per cent on the 5th finger. As can be seen in Fig. 2, the percentage of "arches" on the 5th finger is very small.

(3) Radial patterns are distributed predominantly on the 2nd finger, but tibial (the equivalent of radial in the hand) patterns are equally preponderant on the 1st toe.[1]

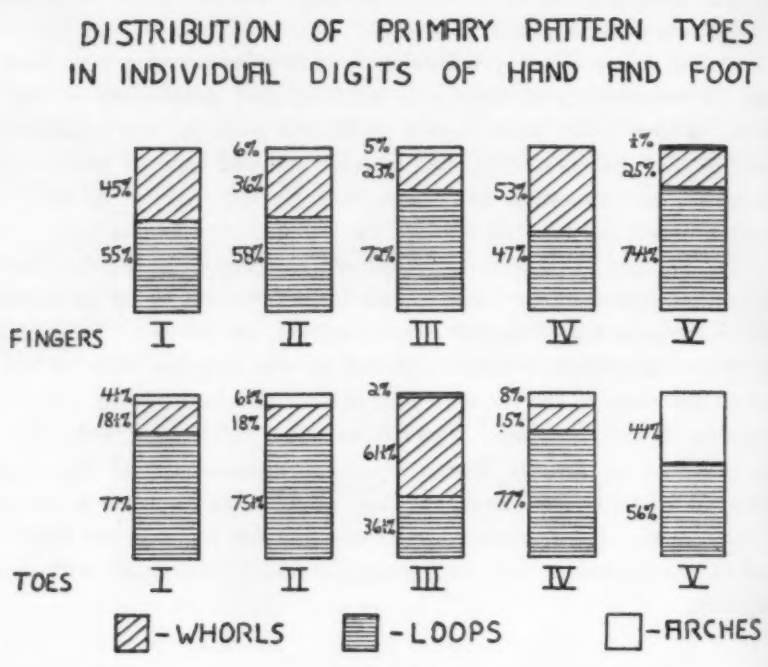

FIGURE 2

(4) The most similar distributions of pattern types are found on digit II of the foot, and digit III of the hand. Other such cross correspondences are found between digit I of the foot and digit II of the hand, digit III of the foot and digit IV of the hand, and digit IV of the foot and digit V of the hand.

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[1]In our series 8.8 per cent of all patterns on fingers are radial patterns (radial "loops" and radially twisted "whorls"), 84.0 per cent of which are on the 2nd finger. Bonnevie (1, p. 12) found a similar distribution of radial "loops" in her series of 24,518 Norwegians. Only 2.6 per cent of the toe patterns are reversed (tibial) in our series, and of these 46.1 per cent are on the great toe. Both Hasebe (10, pp. 80-81) and Takeya (16, pp. 36-37) have figures to substantiate these data on toes.

The implications of these findings have an important bearing on individual finger-toe correlations, and will be elucidated in the next section.

INTRA-INDIVIDUAL CORRELATIONS IN PATTERN EXPRESSION BETWEEN FINGERS AND TOES

1. *Correlation in Pattern Expression Between Individual Digits of Hands and Feet*

As was brought out in the last section, pattern distributions on individual fingers and toes show the greatest similarities between, respectively, digits I, II, III, IV of the foot, and digits II, III, IV, V of the hand. This leaves digit I of the hand and digit V of the foot without homologues. Thus the comparisons of the 8 digits show that if the foot is to be compared with the hand, the foot as a whole must be shifted one digit tibialward.

This shift, it will be noted, is in keeping with the fact that the anatomical axis of the foot lies medial to that of the hand in man and in some gorillas (11, pp. 165-71). Additional evidence of such a shift is afforded in Danforth's (6, p. 192) work on the incidence of hair on the 2nd phalanges in fingers and toes of man, and in Roskoschny's (14, p. 350) and Wolff's (18, pp. 74-78) investigations on the typical localization of syndactylism in toes II and III, and fingers III and IV. Our results may be taken as additional evidence that the axis of the foot has been shifted as compared with that of the hand.

So it becomes evident that the only individual digit correlations we are likely to find are those that take this axial shift into consideration. However the only correlations that are statistically significant are between the right 4th toe and the right 5th finger, and between the left 2nd toe and the left 3rd finger. In the first, the difference between the obtained correspondence and the chance correspondence is 3.7 $\times$ the S.E.: in the second, 2.2 $\times$ the S.E.. These correlations are in regard to "whorls" alone. The lack of correlation, except in the above instances, is disturbing, although these correlations as a whole are relatively greater than those existing between corresponding digits of the hands and feet, *e.g.* that between digit I of the hand and digit I of the foot.

These very low correlations are at least partly due to the greater absolute reduction of patterns in the toes. Thus it may be that in

order to discover any individual finger-toe correspondence, it is necessary to compensate in some way for this pattern reduction. It may well be possible to translate toe pattern values into equivalent finger pattern values, and then see if a correlation exists. But because of its own arbitrary nature it is feared that such a procedure would defeat its own ends.

In order to have some figures to compare with these, the difference between chance and obtained correspondences in "whorls" was worked out between homologous digits of the right and left hands of the same individual. In comparing right and left thumbs, the surplus over and above the chance correspondence is 3.6 × the S.E.; in 2nd fingers, .177 × the S.E. (an extremely low degree of correlation, possibly due in part to the fact that the 2nd finger is the most variable in pattern expression); in 3rd fingers, 6.37 × the S.E.; in 4th fingers, 3.13 × the S.E.; in 5th fingers 3.41 × the S.E.. All these correspondences, except for that in the 2nd fingers, are well above the requirements for statistical significance. Hence there is a well defined bimanual correspondence in pattern type between homologous fingers.

A similar procedure was applied to pattern occurrence in homologous toes of the right and left feet in the same individual. In regard to incidence of "whorls" on right and left great toes, the difference between the chance and the obtained correspondence is 3.5 × the S.E.; in 2nd toes, 6.09 × the S.E.; in 3rd toes, 4.36 × the S.E.; in 4th toes, 5.88 × the S.E.; and in 5th toes there are no "whorls". So there is an even greater bilateral correspondence in homologous toes than in homologous fingers of the same individual. This finding will be brought up again in connection with expression of bilateral asymmetry in fingers and toes.

In comparing these data with those concerned with finger-toe correlation, it is quite evident that within the same genetic complex (the same individual) the degree of similarity in pattern expression between bilaterally homologous structures is very much greater than that shown between serially homologous structures.

2. *Correlations in Pattern Expression Between Total Digits of Hands and Feet*

Since there is little or no digit-for-digit correlation between hands and feet in the same individual, the possibility suggests itself of an intra-individual correlation in pattern expression between total digits

of the hands and total digits of the feet. These correlations are as follows: with regard to "whorls" and their ocurrence on fingers of both hands as against their occurrence on toes of both feet in the same individual, the coefficient of correlation is .418 ± a S.E. of 0.852; with regard to simultaneous occurrence of "loops", the c. of c. is .333 ± a S.E. of 0.0889; and with regard to "arches", the c. of c. is .1698 ± a S.E. of 0.0971 (not a statistically significant correlation). Since "whorls" show the highest correlations, those of other patterns will be disregarded from now on.

Since the matter of bilateral asymmetry is one of the considerations of this paper, it is interesting to compare intra-individual correlations between total fingers and total toes with intra-individual correlations between fingers and toes of separate hands and feet. These correlations, ranged according to their size, are: right hand and right foot; right hand and left foot; left hand and left foot; and lowest, left hand and right foot. The exact figures are given in Table 1. From these data we may infer that pattern distributions on toes of both right and left feet are more similar to those of right hands than to those of left hands.

Even though these intra-individual correlations are low, that they are real is shown by the following procedure. Dividing the 100 individuals of our series into two sets of 50 on a pure chance basis, and determining the degree of correlation between the fingers and the toes of the randomly picked pairs from each group of 50, no correlation even remotely approaching statistical significance was obtained.

3. *Bimanual and Bipedal Correlations in Pattern Expression*

In order to estimate the significance of these intra-individual correlations in serially homologous structures, they are compared with intra-individual correlations between bilateral structures, such as the right and left hands, and the right and left feet in the same individual. Here the coefficient of correlation in "whorls" between fingers of right and left hands of the same individual is .889 ± 0.0209 S.E.. This is about the expected correlation between bilateral structures. The coefficient of correlation in "whorls" between toes of right and left feet is somewhat lower —.7848 ± 0.0385 S.E.. At present we have no theory to account for this lower correlation in feet.

A special case of intra-individual correlation is seen in identical twins, which may be thought of as derived from one zygote divided in more or less equivalent parts. The data for these correlations are

TABLE 1

Intra-Individual Correlations in Pattern Expression

PATTERN	CASE	C. OF CORREL.	S. E.
"Whorls"	fingers of identical twins (50)	.976	±.00669
"Whorls"	intra-individual right & left hands	.889	±.02096
"Whorls"	intra-individual right & left feet	.784	±.03850
"Whorls"	fingers of fraternal twins (50)	.5606	±.06857
"Whorls"	intra-individual R. hand & R. foot	.4334	±.08121
"Whorls"	intra-individual all fingers & all toes	.418	±.08527
"Whorls"	intra-individual R. hand & L. Foot	.3905	±.08465
"Whorls"	intra-individual L. hand & L. foot	.3519	±.08761
"Loops"	intra-individual all fingers & all toes	.3333	±.08891
"Arches"	intra-individual all fingers & all toes	.1688	±.09715
"Whorls"	intra-individual L. hand & R. foot	.1260	±.09841
"Whorls"	random pairs total toes	.0575	±.19338
"Whorls"	random pairs total fingers	—.0290	±.14170

taken from Newman (13, p. 438). Here the asymmetry mechanism is eliminated from showing an effect, because in each pair of twins all fingers of twin A are compared with all fingers of twin B. The coefficient of correlation of pattern expression between members of 50 pairs of identical twins is .976 ± 0.00669 S.E.. This extremely high correlation seems to indicate that if the epidermal patterns themselves are not almost completely genetically predetermined, at least the growth behaviors of the volar pads are.

A final comparison suggests itself. It is concerned with correlations in bilaterally homologous structures between genetically different individuals. Once more we can draw vital data from Newman's twin studies. Here we have chosen 50 pairs of same-sexed fraternal twins—which are genetically equivalent to siblings. As in the identical twins, the total fingers of twin A are compared with the total fingers of twin B. The coefficient of correlation is .5606 ± 0.0685 S.E.. This is about the expected degree of correlation to be found in same-sexed siblings. It is evident now that the degree of correlation between all fingers and toes of the same individual is considerably less than the correlation between all fingers of individuals as genetically different as siblings. This, again, is probably due to the fact that foot differentiation lags behind that of the hand, causing pattern expression on the toes to be considerably suppressed.

By way of summary, the various correlations are listed in order of their size, beginning with the highest and ending with the lowest.

The significance of this descending order of degrees of correlation in some instances seems to offer too difficult a problem for us to attempt to solve at present.

EXPRESSION OF BILATERAL ASYMMETRY IN PATTERN

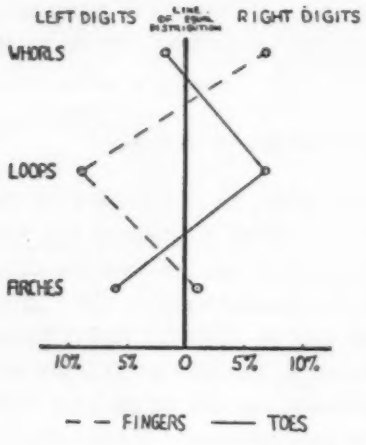
The unequal distribution of pattern types on the fingers of the right and left hands as an expression of bilateral asymmetry has long been recognized. The results of Newman (12, pp. 306-11) for right-handers, in brief, are as follows: "whorls" predominate on right hands over left hands in the ratio of 55.48 per cent to 44.52 per cent; "loops" predominate on left hands over right hands in the ratio of 51.9 per cent to 48.1 per cent; and "arches" predominate on left hands over right hands in the ratio of 66.7 per cent to 33.3 per cent. Similar figures taken from Bonnevie's (1, p. 12) huge series substantiate these findings.

An analysis of our finger print series gives much the same results. In our series, however, "arches" are slightly preponderant on the right instead of the left hand. It is important to note that "arches" are a relatively rare pattern on fingers, and hence a series of 100 may not be large enough to demonstrate true distribution. This somewhat anomalous situation probably makes little difference in the main results, since we are comparing pattern distribution in fingers and toes of the same individual (100 cases).

It might have been expected that the toes of the same individuals would show the same direction of bilateral asymmetry as their fingers. Toes, however, show a definite statistical reversal of asymmetry as compared to fingers. This reversal holds good for the distribution of all three main pattern types. Thus, "whorls" predominate on the left foot in the ratio of 50.7 per cent to 49.3 per cent; "loops" predominate on the right foot in the ratio of 53.4 per cent to 46.6 per cent and "arches" predominate on the left foot in the ratio of 61.5 per cent to 38.5 per cent. In each case the swing of pattern preponderance is in the opposite direction to that of fingers. This reversal in toes

ASYMMETRY REVERSAL IN TOE PRINTS
AS COMPARED WITH FINGER PRINTS

GRAPH



TABULATION

PATTERN	DIGITS	LEFT	RIGHT
WHORLS	FINGERS	32.4	40.2
	TOES	23.0	22.2
LOOPS	FINGERS	65.8	57.0
	TOES	61.0	67.8
ARCHES	FINGERS	1.8	2.8
	TOES	16.0	10.0

FIGURE 3 and TABLE 2

is substantiated in every case by Hasebe's (10, p. 80) and Takeya's (16, p. 36) figures.

So the asymmetry graph shown in toes is almost a mirror image of that shown in fingers. Fig. 3 and Table 2 show this imaging more concretely.

These predominances of pattern types are not large, but because we are dealing with finger and toe prints of the same individuals, they acquire added value and significance.

It is interesting to note that the more nearly symmetrical distribution of patterns on toes as compared with fingers is supported by the correlations given earlier in the paper. However the somewhat lower right foot—left foot correlation is hard to reconcile with the above findings.

Incidently it should be mentioned that the toe patterns of right-handers exhibit almost the same direction and degree of asymmetry as do the fingers of left-handers. It would be interesting to know how the asymmetry runs in the toe patterns of left-handers.

These comparative data naturally suggest some interesting biological problems concerning handedness and footedness. It is best, however, in a paper such as this, to disregard the more theoretical implications of the analyses.

A RACIAL COMPARISON IN DISTRIBUTION OF TOE PATTERNS

It has been known for some time that finger patterns of different races show considerable variation. Bonnevie (1, pp. 17-23) has tabulated the work of a number of investigators who collected finger print data from different races, and this tabulation shows quite striking differences in pattern distribution. Geipel (9, p. 57) has recently enlarged upon Bonnevie's summary of salient racial finger print data.

Racial differences in toe prints have been noted briefly by Takeya, who compared his series of 1000 North Chinese males with Hasebe's series of 100 Japanese males. As shown in Table 3, he found that 20.7 per cent of the North Chinese toe patterns are "whorls", as against 17.2 per cent "whorls" on Japanese toes. There are 59.8 per cent "loops" for North Chinese toes, and 61.0 per cent for Japanese. In distribution of "arches", the figures are 19.4 per cent for North Chinese, and 21.8 per cent for Japanese. So according to these data, the toes of Japanese show a slightly higher incidence of reduced patterns as compared with those of North Chinese.

We have taken the opportunity to compare these data with the findings for our series of 100 European-Americans. Generally speaking, the toe patterns of European-Americans have a higher percentage of more highly expressed patterns than do those of North Chinese and Japanese. This is evidenced in the 22.6 per cent "whorls", 64.4 per cent "loops", and 13.0 per cent "arches" in the European-Americans

TABLE 3

Pattern Distribution on Fingers and Toes of Japanese, Chinese, and European-Americans

PATTERN	TOES			FINGERS		
	Japanese (Hasebe)	Chinese (Takeya)	European-American	European-American	Japanese (Hasebe)	Chinese (Kubo)
"Whorls"	17.2	20.7	22.6	36.3	43.6	50.7
"Loops"	61.0	59.8	64.4	61.4	53.6	47.7
"Arches"	21.8	19.4	13.0	2.3	2.8	1.4

of our series. As can be seen in Table 3, European-Americans have the highest incidence of highly expressed patterns on toes, North Chinese are intermediate in this respect, and Japanese have the greatest percentage of reduced patterns.

This situation, while not at all strange in itself, becomes very curious in the light of a comparison between our finger print series and the finger print series of Hasebe (10, pp. 38-49) for 276 Japanese. It is to be regretted that Hasebe's finger print data are not compiled so that they can be matched individually with his toe print data. Nevertheless the comparison is still interesting.

While the fingers of Japanese show a higher degree of pattern *expression* than do the fingers of our European-Americans, the toes of Japanese show a much higher degree of pattern *reduction* than that shown in toes of European-Americans. This means that the ratio between pattern expression in fingers and toes of Japanese is much more unequal than in European-Americans. This is best shown in

TABLE 4

Adopted System of Finger and Toe Pattern Classification*

Whorls	
W —Symmetrical whorl	Concentric formation with no twist of total pattern
W^r —Radially twisted whorl	Whole pattern with definite radially directed twist
W^u —Ulnarly twisted whorl	Whole pattern with definite ulnarly directed twist
W^d—Symmetrical double loop	Neither loop twisted
W^{dr}—Radially twisted double loop.....	Distinguished from W^{r-u} in that the two cores can be easily traced to the exterior of the pattern
W^{du}—Ulnarly twisted double loop.....	Same as for W^{dr}
W^{pr}—Radially opening pocket loop.....	Minimal whorl with its triradius no further than 3 ridges (inclusive) from its core, engulfed by a large loop.
W^{pu}—Ulnarly opening pocket loop.....	Same as for W^{pr}
Unusual Patterns	
$\frac{W}{R}$ or $\frac{W}{U}$ —	An elongated crescentic whorl, usually bounded proximally by a small radial or ulnar loop
$\frac{W^d}{U}$ or $\frac{W^d}{R}$ —Triple loop	
W^{rc}—Crested radial loop	Where a series of ridges coming over from the side of the triradius engulf the loop, thereby forming a "crest" over it
W^{uc}—Crested ulnar loop	Same as for W^{rc}
W^{ri}—Invaded radial loop	In which, while the pattern of the loop is engulfed or "invaded", no definite crest is formed
W^{ui}—Invaded ulnar loop	Same as for W^{ri}

*—In toes, tibial corresponds to radial, fibular to ulnar.

TABLE 4 (*con't.*)

Loops	R^u or R^r—Simple radial loop	With a twist (radial or ulnar)
	U^u or U^r—Simple ulnar loop	With a twist (radial or ulnar)
	R —Simple radial loop	
	U —Simple ulnar loop	
	R^a—Minimal radial loop	The triradius touching the inner ridges of the core
	U^a—Minimal ulnar loop	Same as for R^a
Arches	A^t—Tented arch	With a median triraduis and a vertical spine eminating from it
	A —Simple arch	No positive pattern

Table 3. The same ratio of discrepancy in degree of pattern expression holds in a comparison of finger and toes of Chinese and European-Americans, but does not follow in a comparison of Chinese and Japanese finger and toe patterns.

In conclusion, on the basis of racial differences shown in these three toe print series, it seems reasonable to believe that toe prints show racial differences just as clearly as do the dermatoglyphics of the finger, palms and soles.

SUMMARY

(1) This study is based on the finger and toe prints of 100 right-handed European-American males of North and Central European stock, supplementing Hasebe's study of toe patterns of 100 Japanese males and Takeya's investigation of toe patterns of 1000 North Chinese males.

(2) Due to the greater embryonic foreshortening in toe pads, the ridge patterns of toes are flatter and broader than those of fingers.

(3) Correlated with the fact that the differentiation of the foot lags behind that of the hand in embryonic development, pattern expression of the toes is considerably suppressed as compared with that of the fingers, i.e. there is a greater preponderance of reduced patterns on toes. This difference in pattern expression may also be associated with unlike contours of finger and toe pads, independent of the time relations.

(4) This reduction in pattern expression in toes is not evenly distributed on the 10 digits. Thus digit III shows the largest per-

TABLE 5

Types of Patterns in Fingers of 100 European-American Males

TABLE 5

Types of Patterns in Fingers of 100 European-American Males

PATTERN TYPE	LEFT							RIGHT							TOTAL R & L
	I	II	III	IV	V	Total Left	in %	I	II	III	IV	V	Total Right	in %	
A	0	3	4	0	1	8	1.6	0	5	6	0	0	11	2.2	1.9
A^t	0	1	0	0	0	1	.2	0	3	0	0	0	3	.6	.4
Total Arch	0	4	4	0	1	9	1.8	0	8	6	0	0	14	2.8	2.3
U^s	0	4	1	1	0	6	1.2	0	5	2	1	0	8	1.6	1.4
U	63	34	71	54	76	298	59.6	47	25	66	38	73	249	49.8	54.7
R^s	0	4	1	0	0	5	1.0	0	3	1	0	0	4	.8	.9
R	0	18	1	1	0	20	4.0	0	23	1	0	0	24	4.8	4.4
Total Loop	63	60	74	56	76	329	65.8	47	56	70	39	73	285	57.0	61.4
W^{ul}	4	0	0	2	3	9	1.8	6	1	0	2	3	12	2.4	2.1
W^{uc}	0	0	0	1	1	2	.4	0	0	0	0	0	0	0	.2
W^{rl}	0	1	0	0	0	1	.2	0	0	0	0	0	0	0	.1
W^{rc}	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
W^{du}	12	0	1	1	1	15	3.0	6	0	0	2	1	9	1.8	2.4
W^{dr}	0	1	0	0	0	1	.2	0	3	1	1	0	5	1.0	.6
W^d	0	0	0	0	0	0	0	0	1	0	0	0	1	.2	.1
W^{pu}	0	1	0	8	1	10	2.0	0	2	1	7	5	15	3.0	2.5
W^{pr}	0	4	0	0	0	4	.8	0	1	1	0	0	2	.4	.6
W^u	16	11	14	21	10	72	14.4	31	11	14	24	13	93	18.6	16.5
W^r	0	8	2	0	0	10	2.0	2	10	0	0	0	12	2.4	2.2
W	5	11	5	11	6	38	7.6	10	7	5	25	5	52	10.4	9.0
Total Whorl	37	37	22	44	22	162	32.4	55	36	22	61	27	201	40.2	36.3

TABLE 6

Types of Patterns in Toes of 100 European-American Males

PATTERN TYPE	LEFT							RIGHT							TOTAL R & L
	I	II	III	IV	V	Total Left	in %	I	II	III	IV	V	Total Right	in %	
A	6	7	4	10	52	79	15.8	3	6	0	6	35	50	10.0	12.9
A^t	0	0	0	0	1	1	.2	0	0	0	0	0	0	0	.1
Total Arch	6	7	4	10	53	80	16.0	3	6	0	6	35	50	10.0	13.0
F^a	4	2	3	4	6	19	3.8	3	0	1	1	9	14	2.8	3.3
F	60	74	32	71	41	278	55.6	78	75	34	76	56	329	63.8	58.7
F^t	0	0	0	1	0	1	.2	0	0	0	0	0	0	0	.1
T^a	0	0	0	0	0	0	0	0	0	0	1	0	1	.2	.1
T	5	0	0	0	0	0	1.0	4	0	1	0	0	5	1.0	1.0
T^t	0	0	2	0	0	2	.4	0	0	0	0	0	0	0	.2
Total Loop	69	76	37	76	47	305	61.0	85	75	36	78	65	339	67.8	64.4
W^{ft}	3	0	1	0	0	4	.8	3	1	1	0	0	5	1.0	.9
$\frac{W}{F}$	0	1	4	0	0	5	1.0	0	0	0	0	0	0	0	.5
W^{dt}	4	12	48	8	0	72	14.4	1	11	48	8	0	68	13.6	14.0
W^{dt}	2	0	1	3	0	6	1.2	1	1	0	4	0	6	1.2	1.2
W^d	0	0	0	0	0	0	0	1	0	0	0	0	1	.2	.1
$\frac{W^d}{F}$	0	0	0	1	0	1	.2	0	0	1	0	0	1	.2	.2
W^{pt}	1	0	0	0	0	1	.2	0	0	0	0	0	0	0	.1
W^t	7	4	4	1	0	16	3.2	1	2	6	2	0	11	2.2	2.7
W^t	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
W	8	0	1	1	0	10	2.0	5	4	8	2	0	19	3.8	2.9
Total Whorl	25	17	58	14	0	115	23.0	12	19	64	16	0	111	22.2	22.6

centage of highly expressed patterns ("whorls") and digit V shows the highest percentage of reduced patterns ("arches").

(5) Reversed (radial on hand, and tibial on foot) patterns are found mainly on digit I of the foot, but on digit II of the hand.

(6) With regard to distributions of pattern types, it is evident that the greatest similarities exist between digits I, II, III, and IV of the foot, and digits II, III, IV, and V, respectively, of the hand. This indicates that if individual digits of the hands and feet are to be compared, the axis of the foot must be shifted one digit medially. Such a finding is consistent with the observations of investigators in other fields, and adds another item to the evidence that there is such a shift in axes.

(7) There is, however, little or no correlation between individual digits of the hands and feet in the same individual, despite the fact that this axial shift was taken into account.

(8) Nevertheless there is an appreciable correlation in pattern expression between total fingers and total toes in the same individual.

(9) This correlation is compared to those between fingers of both hands in the same individual, in identical twins, in fraternal twins, and in a random sample. This comparison suggests that although pad shape and therefore pattern shape or contour is dependent to a large extent upon genetic factors, that the lag in foot differentiation as compared with hand differentiation tends to suppress pattern expression in toes.

(10) Bilateral asymmetry as expressed in toe patterns, mirror images, or is the reverse of, that expressed in finger patterns.

(11) Toe prints of European-Americans, North Chinese, and Japanese show racial differences, and it is found that toes of European-Americans have the most highly expressed patterns, toes of North Chinese are intermediate in number of highly expressed patterns, and toes of Japanese show the largest number of reduced patterns.

(12) Strangely enough the ratio between the degree of pattern expression in fingers and toes of Japanese is much more unequal than that in European-Americans.

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A COMPARATIVE STUDY OF THE MORTALITY OF THE POPULATION OF GREECE[1]

BY VASILIOS G. VALAORAS, M. D., D. P. H.

THE LAST census of Greece was taken on May 15th and 16th of the year 1928, and presented a total population of 6,204,684 inhabitants. The distribution of the population in that year was found to be distorted as to age and sex, there being an unusual excess of children and of females.

This disturbance in the age and sex distribution of the population may be attributed partly to the long wars of the country which started in 1912 with the Balkan wars and continued, with some short intervals between, up to the year 1922, but chiefly was the effect of a mass immigration which occurred as an aftermath of the last war. Following the collapse of the Greek front in Asia Minor in August 1922, a population amounting to one-and-one-half million individuals left hastily their native land in Asia Minor and Eastern Thrace and entered the country as refugees. This refugee population, which represented one-fourth of the total population of the country at that time, was a highly selected group and consequently its influence on the age and sex distribution of the total population would be conspicuous for many years after its arrival.

The general mortality of the country was also affected temporarily by the influx of the refugees. The exposure of this population in a new environment to many unfavorable conditions (lack of immediate and appropriate shelter and food, exposure to epidemics, etc.) resulted in a relatively high mortality among them and their children in the first years after their arrival.

By the year 1928, however, the settlement of the refugee population was almost completed and the acclimatization was relatively far advanced. It would be expected, therefore, that by that year the influence of the new population on the general mortality of the country would

[1]From the Department of Biostatistics, School of Hygiene and Public Health, The Johns Hopkins University, Paper No. 207.

be of little importance. Consequently, the present study which deals with the mortality of the population in Greece in 1928, showed the forces of mortality which were prevailing in the total population without being distorted unduly by the recent immigration.

The best picture of the mortality of a population is, as is well known, given by means of a life table, and this study presents two life tables, one for total males and the other for total females of the population of Greece for the census year 1928. The original information[2] was taken from the official vital statistics of the country, namely the census returns and the mortality statistics. The population for the midyear day of 1928 was estimated by using the material of the last two censuses, 1920 and 1928, and by assuming an arithmetic increase of the population. The number of deaths was determined by averaging the deaths for the five years 1926, 1927, 1928, 1929 and 1930, in order to minimize the influence of the newly settled refugee population as well as the slightly increased mortality which occurred in Greece in the year 1929 from an almost country-wide epidemic of dengue fever.

The data given by this treatment and the redistribution of persons of "unknown age" appear in Table 1.

The method used in the construction of the life tables is one developed by Doctor Lowell J. Reed, Professor of Biostatistics in the School of Hygiene and Public Health of The Johns Hopkins University, which is based on the correlation existing between m_x and q_x in Glover's Life Tables[3] for the United States. This correlation is worked out for the different ages and gives formulae by means of which q_{x+5} for the life table can be predicted from ${}_{10}m_x$ as found in the population under consideration. The graduation of the q_x curve is obtained by fitting two exponential third order parabolas, and welding them at the age of 20 to 25 years.

The complete life tables for males and females in the total population of Greece for the year 1928 follow in the appended tables. The two graphs give a comparison of the mortality rates and the expectation of life for males in a few selected countries, namely New Zealand

[2]Annuaire Statistique de la Grèce. *Athenae, Greece,* 1930 and 1931.

[3]United States Life Tables, 1890, 1901, 1910, and 1901-1910. Prepared by James W. Glover. Government Printing Office, *Washington, D. C.*

TABLE 1

Population and Mortality Statistics upon Which the Life Tables For Greece, 1928 are Based

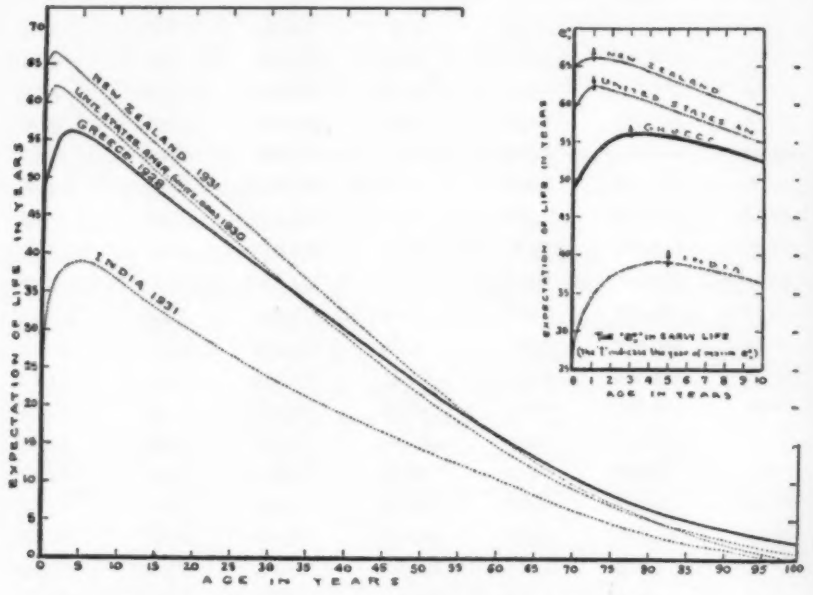
AGE	MALE			FEMALE		
	Population	Deaths	Death-Rate per 1000	Population	Deaths	Death-Rate per 1000
0-1	87,926	9,397	106.87	82,748	8,606	104.00
1-2	69,057	4,592	66.50	64,272	4,316	67.15
2-3	86,239	2,688	31.17	82,904	2,581	31.13
3-4	77,332	1,326	17.15	75,172	1,211	16.11
4-5	73,313	885	12.07	72,149	847	11.74
5-9	321,302	2,021	6.29	306,439	1,820	5.94
10-14	314,311	993	3.16	287,170	905	3.15
15-19	345,083	1,637	4.74	353,163	1,658	4.69
20-24	261,729	1,783	6.81	289,990	1,904	6.57
25-29	255,600	1,820	7.12	266,809	2,075	7.78
30-34	185,743	1,288	6.93	199,532	1,572	7.88
35-39	175,365	1,536	8.76	201,845	1,746	8.65
40-44	152,827	1,447	9.47	175,482	1,329	7.57
45-49	161,265	1,918	11.89	153,454	1,380	8.99
50-54	140,067	2,086	14.89	137,027	1,457	10.63
55-59	108,594	2,331	21.47	101,613	1,442	14.19
60-64	92,669	2,664	28.75	99,556	1,957	19.66
65-69	71,147	2,011	42.32	71,106	2,265	31.85
70-74	49,099	2,792	56.86	54,814	2,650	48.35
75-79	30,086	2,528	84.03	29,603	2,413	81.51
80-84	15,210	1,937	127.35	18,746	2,176	116.08
85-89	6,704	1,184	176.61	6,989	1,312	187.72
90-94	2,659	541	203...	4,185	870	208...
95-99	1,118	267	239...	1,595	374	234...
100 and over	546	103	189...	1,239	198	160...
Total	3,084,991	52,775	17.11	3,137,602	49,064	15.64

(1930),[4] United States of America (1930),[5] Greece (1928) and India (1931).[6]

The comparison of these two graphs reveals two fundamental differences between the countries compared, the first being the year of the highest expectation of life and the second, the general trend of the mortality curves, which cross each other at the later years of age. A discussion of each of these points follows:

THE YEAR OF THE HIGHEST EXPECTATION OF LIFE

The curve of the expectation of life for the human population has a characteristic rise in early life, the expectation of life at birth being lower than that at the ages immediately following birth. This is due



GRAPH 1. THE COMPLETE EXPECTATION OF LIFE—MALES

[4] *Statistical Bulletin,* Metropolitan Life Insurance Company. Vol. 15, May, 1934.

[5] *Statistical Bulletin,* Metropolitan Life Insurance Company. Vol. 16, May, 1935.

[6] Report of Census of India. 1931, Vol. 1, *Delhi, India.*

to the greater danger of dying in the first year of life than in subsequent years. Once the baby has passed through this dangerous period and enters his second year, the probability for a longer life is increased and consequently the expectation of life is higher. The maximum expectation of life occurs between the first and the fifth years of age. The improvement in public health methods of most civilized countries in recent decades has resulted in a tendency to decrease gradually the infant mortality. As an accompaniment of this decrease, the curve of the expectation of life has undergone some interesting changes; the rapid rise at the beginning of the curve has become smaller, while the maximum expectation of life has moved toward the early ages. The upper right corner of Graph 2 shows these changes for four countries differing widely in mortality rates, India having almost the highest recorded rates, and New Zealand the lowest. The change in the first part of the curve may probably be attributed to the saving of infant lives by preventive medicine, while the absolute level of the expectation of life is probably related also to such biological factors as racial vitality, etc.

The magnitude of the rise of the curve in the first years of life may be expressed as a ratio between the expectation of life at birth and the maximum expectation of life, this ratio forming a rough

TABLE 2

The Expectation of Life at Birth Relative to the Maximum Expectation of Life

COUNTRIES	MALE	FEMALE
New Zealand (1931)	$\frac{65.04}{66.61} \times 100 = 97.6$	$\frac{67.88}{68.64} \times 100 = 98.9$
United States (1930)	$\frac{59.31}{62.12} \times 100 = 95.5$	$\frac{62.83}{64.99} \times 100 = 96.7$
Greece (1928)	$\frac{49.09}{56.01} \times 100 = 87.6$	$\frac{50.89}{58.07} \times 100 = 87.6$
India (1931)	$\frac{26.91}{38.96} \times 100 = 69.1$	$\frac{26.56}{36.75} \times 100 = 72.3$

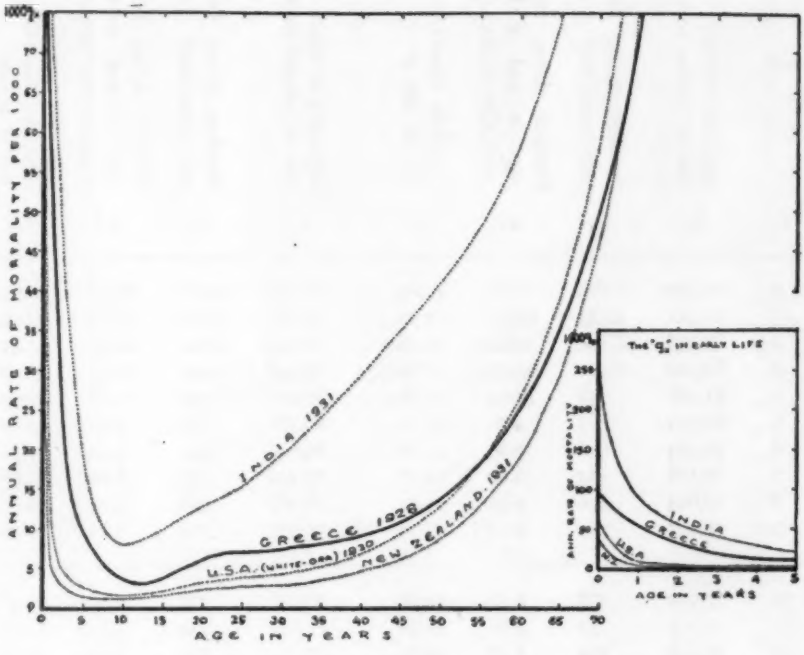
measure of the accomplishment in reducing infant mortality. Table 2 gives this ratio in percentage form for the four countries compared in Graph 1.

It is obvious that if the maximum expectation of life were at birth, the index would be one hundred, and this would represent the elimination of the excessive risk of dying in the first year of life. The difference between the expectation of life at birth and the maximum expectation is associated with the number of years elapsed from birth till the maximum expectation occurs, larger differences being met where the maximum expectation of life is more retarded. New Zealand (white population) is considered today to have the most favorable record of longevity, and this achievement is reflected in the fact that the ratio of expectation of life at birth to the maximum expectation is 97.6 for males and 98.9 for females. The United States comes second among the countries studied, and India is last with an index of 69.1 for males and 72.3 for females. It is interesting to note that the index for females runs a little higher than for males, which would be expected from the different levels of mortality rates.

MORTALITY IN OLD AGES ASSOCIATED WITH THAT IN EARLY LIFE

In Graph 2, the mortality rate, q_x, is shown for the four different countries. It is seen that the curve for Greece stands at a higher level than that for the United States or New Zealand in the first half of the life span but lower in the second half, crossing the curves for the two countries at ages 54 and 69 respectively. Thus for these curves a higher mortality in the young ages is associated with a lower mortality in the old ages. This same fact is frequently observed either for different countries compared for about the same time or for the same country compared for different times. Although this crossing in the mortality curves is not observed universally in every comparison, it is met frequently enough in the international bibliography of human mortality to suggest a real association.

The alteration in the mortality curve for any population must be a complex of many biological factors, and the relationship noted here suggests a balancing of the forces of mortality at different ages, which may have a fundamental bearing on the possible increase in human longevity.



GRAPH 2. ANNUAL RATE OF MORTALITY PER THOUSAND MALES

TABLE 3

Life Tables for Greece, 1928

	MALE POPULATION				FEMALE POPULATION			
AGE	Survivors at exact age x out of 100,000 born alive	Number dying between age x and $x+1$	Number dying between age x and $x+1$ per 1000 alive at age x	Complete expectation of life at age x	Survivors at exact age x out of 100,000 born alive	Number dying between age x and $x+1$	Number dying between age x and $x+1$ per 1000 alive at age x	Complete expectation of life at age x
x	l_x	d_x	q_x	e_x°	l_x	d_x	q_x	e_x°
0	100,000	9,509	95.09	49.09	100,000	9,312	93.12	50.89
1	90,491	4,933	54.51	53.22	90,688	4,990	55.02	55.09
2	85,558	2,639	30.84	55.26	85,698	2,640	30.80	57.27
3	82,919	1,432	17.27	56.01	83,058	1,347	16.22	58.07
4	81,487	948	11.64	55.98	81,711	930	11.38	58.02
5	80,539	765	9.50	55.63	80,781	743	9.20	57.69
6	79,774	606	7.60	55.16	80,038	594	7.42	57.22
7	79,168	483	6.10	54.58	79,444	477	6.00	56.64
8	78,685	390	4.95	53.91	78,967	388	4.91	55.98
9	78,295	323	4.12	53.18	78,579	323	4.11	55.25
10	77,972	278	3.57	52.40	78,256	279	3.57	54.48
11	77,694	253	3.26	51.58	77,977	254	3.25	53.67
12	77,441	244	3.15	50.75	77,723	243	3.13	52.85
13	77,197	249	3.22	49.91	77,480	246	3.18	52.01
14	76,948	264	3.43	49.07	77,234	259	3.36	51.18
15	76,684	288	3.75	48.24	76,975	281	3.65	50.35
16	76,396	317	4.15	47.42	76,694	308	4.02	49.53
17	76,079	350	4.60	46.61	76,386	340	4.45	48.73
18	75,729	385	5.08	45.83	76,046	375	4.93	47.94
19	75,344	418	5.55	45.06	75,671	407	5.38	47.18
20	74,926	449	5.99	44.31	75,264	439	5.84	46.43
21	74,477	477	6.40	43.57	74,825	471	6.30	45.70
22	74,000	492	6.65	42.85	74,354	494	6.64	44.98

TABLE 3 *(cont.)*

Life Tables for Greece, 1928

AGE	MALE POPULATION				FEMALE POPULATION			
	Survivors at exact age x out of 100,000 born alive	Number dying between age x and $x+1$	Number dying between age x and $x+1$ per 1000 alive at age x	Complete expectation of life at age x	Survivors at exact age x out of 100,000 born alive	Number dying between age x and $x+1$	Number dying between age x and $x+1$ per 1000 alive at age x	Complete expectation of life at age x
x	l_x	d_x	q_x	e_x°	l_x	d_x	q_x	e_x°
23	73,508	498	6.77	42.13	73,860	509	6.89	44.28
24	73,010	498	6.82	41.41	73,351	519	7.08	43.59
25	72,512	498	6.87	40.70	72,832	528	7.25	42.89
26	72,014	501	6.96	39.97	72,304	536	7.41	42.20
27	71,513	504	7.05	39.25	71,768	542	7.55	41.51
28	71,009	508	7.15	38.52	71,226	547	7.68	40.83
29	70,501	511	7.25	37.80	70,679	551	7.80	40.14
30	69,990	515	7.36	37.07	70,128	554	7.89	39.45
31	69,475	520	7.48	36.34	69,574	555	7.98	38.76
32	68,955	525	7.61	35.61	69,019	556	8.06	38.07
33	68,430	530	7.75	34.88	68,463	556	8.12	37.37
34	67,900	536	7.89	34.15	67,907	555	8.18	36.67
35	67,364	543	8.06	33.42	67,352	554	8.22	35.97
36	66,821	550	8.23	32.69	66,798	552	8.26	35.27
37	66,271	559	8.43	31.95	66,246	550	8.30	34.55
38	65,712	568	8.65	31.22	65,696	548	8.34	33.84
39	65,144	579	8.88	30.49	65,148	545	8.37	33.12
40	64,565	590	9.14	29.76	64,603	543	8.41	32.40
41	63,975	603	9.43	29.03	64,060	542	8.45	31.67
42	63,372	618	9.75	28.75	63,518	540	8.51	30.93
43	62,754	634	10.10	27.57	62,978	540	8.58	30.45
44	62,120	651	10.84	26.85	62,438	541	8.66	29.45

TABLE 3 *(cont.)*

Life Tables for Greece, 1928

AGE	MALE POPULATION				FEMALE POPULATION			
	Survivors at exact age x out of 100,000 born alive	Number dying between age x and $x+1$	Number dying between age x and $x+1$ per 1000 alive at age x	Complete expectation of life at age x	Survivors at exact age x out of 100,000 born alive	Number dying between age x and $x+1$	Number dying between age x and $x+1$ per 1000 alive at age x	Complete expectation of life at age x
x	l_x	d_x	q_x	e_x°	l_x	d_x	q_x	e_x°
45	61,469	670	10.91	26.13	61,897	542	8.76	28.70
46	60,799	691	11.37	25.41	61,355	546	8.89	27.95
47	60,108	714	11.88	24.70	60,809	551	9.05	27.20
48	59,394	739	12.45	23.99	60,258	557	9.25	26.44
49	58,655	766	13.07	23.28	59,701	566	9.49	25.69
50	57,889	795	13.74	22.58	59,135	578	9.77	24.93
51	57,094	827	14.48	21.89	58,557	592	10.11	24.17
52	56,267	861	15.30	21.21	57,965	609	10.51	23.41
53	55,406	893	16.11	20.53	57,356	630	10.98	22.65
54	54,513	935	17.15	19.86	56,726	653	11.52	21.90
55	53,578	975	18.20	19.19	56,073	681	12.15	21.15
56	52,603	1,018	19.34	18.54	55,392	713	12.88	20.40
57	51,585	1,062	20.59	17.90	54,679	750	13.71	19.66
58	50,523	1,109	21.94	17.26	53,929	790	14.66	18.92
59	49,414	1,157	23.41	16.64	53,139	836	15.73	18.20
60	48,257	1,207	25.01	16.03	52,303	887	16.95	17.49
61	47,050	1,258	26.73	15.42	51,416	942	18.31	16.78
62	45,792	1,309	28.59	14.83	50,474	1,002	19.85	16.08
63	44,483	1,362	30.61	14.26	49,472	1,067	21.56	15.40
64	43,121	1,414	32.79	13.68	48,405	1,136	23.47	14.73
65	41,707	1,465	35.14	13.14	47,269	1,210	25.60	14.07

TABLE 3 *(cont.)*

Life Tables for Greece, 1928

AGE	MALE POPULATION				FEMALE POPULATION			
	Survivors at exact age x out of 100,000 born alive	Number dying between age x and $x+1$	Number dying between age x and $x+1$ per 1000 alive at age x	Complete expectation of life at age x	Survivors at exact age x out of 100,000 born alive	Number dying between age x and $x+1$	Number dying between age x and $x+1$ per 1000 alive at age x	Complete expectation of life at age x
x	l_x	d_x	q_x	e_x°	l_x	d_x	q_x	e_x°
66	40,242	1,516	37.67	12.60	46,059	1,287	25.96	13.42
67	38,726	1,564	40.39	12.07	44,772	1,368	30.56	12.80
68	37,162	1,610	43.32	11.56	43,404	1,451	33.44	12.18
69	35,552	1,652	46.48	11.06	41,953	1,536	36.61	11.59
70	33,900	1,690	49.87	10.57	40,417	1,620	40.09	10.99
71	32,210	1,723	53.50	10.10	38,797	1,703	43.91	10.45
72	30,487	1,750	57.40	9.64	37,094	1,784	48.09	9.91
73	28,737	1,770	61.59	9.20	35,310	1,859	52.66	9.38
74	26,967	1,782	66.06	8.77	33,451	1,928	57.64	8.87
75	25,185	1,785	70.86	8.36	31,523	1,988	63.07	8.39
76	23,400	1,778	75.99	7.96	29,535	2,037	68.98	7.92
77	21,622	1,762	81.47	7.57	27,498	2,073	75.40	7.47
78	19,860	1,734	87.33	7.20	25,425	2,094	82.37	7.03
79	18,126	1,696	93.59	6.84	23,331	2,098	89.92	6.62
80	16,430	1,647	100.26	6.49	21,233	2,083	98.08	6.73
81	14,783	1,587	107.38	6.16	19,150	2,047	106.92	8.85
82	13,196	1,517	114.97	5.84	17,103	1,992	116.45	5.49
83	11,679	1,437	123.05	5.53	15,111	1,915	126.74	5.15
84	10,242	1,348	131.66	5.24	13,196	1,819	137.82	4.82
85	8,894	1,252	140.82	4.96	11,377	1,704	149.75	4.51
86	7,642	1,151	150.66	4.69	9,673	1,573	162.59	4.22
87	6,491	1,044	160.91	4.43	8,100	1,429	176.38	3.94

TABLE 3 *(cont.)*

Life Tables for Greece, 1928

AGE	MALE POPULATION				FEMALE POPULATION			
	Survivors at exact age x out of 100,000 born alive	Number dying between age x and $x+1$	Number dying between age x and $x+1$ per 1000 alive at age x	Complete expectation of life at age x	Survivors at exact age x out of 100,000 born alive	Number dying between age x and $x+1$	Number dying between age x and $x+1$ per 1000 alive at age x	Complete expectation of life at age x
x	l_x	d_x	q_x	e_x°	l_x	d_x	q_x	e_x°
88	5,447	936	171.92	4.18	6,671	1,275	191.18	3.68
89	4,511	828	183.61	3.95	5,396	1,117	207.07	3.43
90	3,683	722	196.02	3.72	4,279	959	224.09	3.19
91	2,961	619	209.19	3.51	3,320	804	242.33	2.97
92	2,342	523	223.16	3.30	2,516	659	261.85	2.76
93	1,819	433	237.97	3.11	1,857	525	282.72	2.57
94	1,386	352	253.66	2.92	1,332	406	305.03	2.38
95	1,034	279	270.29	2.75	926	305	328.86	2.20
96	755	217	287.90	2.58	621	220	354.30	2.04
97	538	165	306.55	2.42	401	153	381.43	1.89
98	373	122	326.28	2.28	248	102	410.35	1.74
99	251	87	347.02	2.13	146	64	441.17	1.61
100	164	61	369.21	1.99	82	39	473.99	1.49
101	103	40	392.54	1.88	43	22	508.87	1.39
102	63	26	417.19	1.76	21	11	546.06	1.33
103	37	16	443.23	1.65	10	6	585.56	1.20
104	21	10	470.73	1.52	4	2	627.52	1.25
105	11	5	499.76	1.45	2	1	672.09	1.00
106	6	3	530.40	1.17	1	1	719.41	1.00
107	3	2	562.73	1.00	....	....	.....	...
108	1	1	596.82	1.00	....	....	.....	...

THE RELATIONSHIP BETWEEN THE LENGTH AND WEIGHT OF THE NEW-BORN INFANT AND THE HEIGHT AND WEIGHT OF THE MOTHER

BY BORGHILD GUNSTAD AND ALAN E. TRELOAR

Department of Biometry, University of Minnesota

THE foundations for quantitative study of the influence of heredity in determining the physical dimensions of man were laid with enduring strength by Francis Galton some fifty years ago when he formulated the coefficients of correlation and regression. Marshalling facts flowing from application of his statistical reasoning in an epoch making dissertation entitled "Natural Inheritance" (1889), he pointed a way and commenced blazing a trail that has been expanded into one of the great highways, from a methodological point of view, in modern scientific analysis. This work was primarily responsible for drawing the tremendous energy and intellectual power of Karl Pearson, then Profesor of Mechanics and Applied Mathematics at University College, London, to the biometric frontier.

Galton had convincingly demonstrated by his technique a very definite correspondence between the physical characteristics of parents and their adult offspring, and had given a unique measure of its intensity. Pearson began immediately to explore more fully and extend this concept of correlation in the analysis of problems of heredity, and in 1895 presented his study (with Alice Lee) of "Regression, Heredity and Panmixia," in which he dealt at length with 200 family records compiled by Galton. Supplementing Galton's data by a much larger series of measurements on blood relatives in man, Pearson and Lee later (1903) published a classical study of the laws of inheritance of physical characters in man.

This *opus* was based on more than a thousand records of stature, span, and length of forearm for parents and offspring. They found correlations of .494 and .507 between stature of mother and stature of adult son and daughter, respectively. The correlations for the other two less complex characters, span

and forearm, were somewhat lower. The relationships between weight of parent and weight of offspring were not investigated, perhaps because weight is far more subject to modification through nutritional conditions and environmental forces than stature. That there is a correlation of approximately .50 between the stature of mother and stature of adult son or daughter was definitely established, however, for English families.

There have been no such extensive investigations of the relationship between the height and weight of the mother and the corresponding measurements of the infant at birth.

Aiko (1928), in a study of the Japanese newly born infant, did consider the associations between the length and weight of the child and the height and weight of the mother. He did not give any numerical evaluation of the correlations observed, however, nor did he discuss his results to any extent. He simply determined that there is *some* degree of correspondence between the measurements of the child at birth and the height and weight of the mother.

Ballantyne and Brown (1922), in a study of the postmature infant, recognized that weight alone was not a satisfactory criterion of postmaturity, since the infant might be larger than average merely because its father or mother was above average in size. They stated: "There is good reason to suppose that paternal heredity is one of the factors determing the weight of the infant at birth."

Pinard (see Newman, 1906, p. 81) concluded that the average weight at birth of infants of working mothers was greater if the mother spent part or all of the pregnancy period in the hospital. The average weight of 500 infants delivered by mothers who worked through the whole period of pregnancy was 3000 gm. The average weight of 500 infants born to mothers who spent the last few months of the period of gestation in the hospital was 3290 gm., and 500 mothers who spent the whole pregnancy period in the hospital bore infants that averaged 3368 gm. in weight. Pinard attributed this increase in average weight of infants delivered by working mothers who spent part or all of the period of pregnancy in the hospital to rest and improved nutrition.

Prochownik (see Newman, 1906, p. 84) claimed demonstration of a direct relationship between the nutrition of the mother and the weight of the infant, indicating that it was possible to reduce the weight of the child at birth by restricting the mother's diet. On the other hand, according to Slemons and Fagan (1927), the infant's birth weight can not be accurately regulated by the mother's diet. They concluded, however, that diet control will serve to prevent overgrowth of the fetus, and thus to lessen the hazard of birth injuries both to the mother and to the child.

Comparatively little has been done in the form of full statistical analysis of extensive records of the relationship between the maternal height and weight and the corresponding measurements of the newborn child. This is rather surprising because interest in such relationships should be considerable, not only to the students of anthropometry

and heredity, but also to obstetricians. To the former groups there should be added appeal in such studies because of the relative constancy of the environmental forces affecting the offspring. The practical consequences of any degree of relationship which exists might well be quite important to those concerned with the problems of delivery. It is therefore thought well worth-while to report here the results of an initial investigation of this problem.

THE DATA

Through the courtesy of Dr. Charles E. Remy and Dr. John A. Urner, Superintendent and Chief Obstetrician respectively of the Minneapolis General Hospital, the records of the obstetrical division of that hospital have been made available to the writers for the purpose of this and other studies. These records are deemed of special interest because of the comparative uniformity of judgment in describing the infants, a feature in the data which flows from the detailed supervision of the staff activities in recent years by Dr. Urner.

All uniparous births during the period January 1, 1930 to December 30, 1932, are included in this series, except where the mother failed to survive the period of hospitalization. Because of the rather empirical nature of the decision as to whether a child was premature, normal, or post term, it was decided to include all infants surviving delivery regardless of their judged stage of maturity. While the weight in grams is available for all infants, length measurements had unfortunately to be discontinued in July of 1930 as a precautionary measure in arresting the spread of skin troubles. Thus, only 444 lengths are available out of the total series of 3845 cases. These measurements of length were made to the nearest quarter centimeter.

Upon discharge from the hospital, all mothers were measured for height and weight under standard conditions by competent persons. As a reflection of the mothers' physical dimensions comparable to those taken on the child, these are the most dependable ones available. Recorded *antepartum* measurements are not wholly acceptable since they represent the mother's own estimate in each case. In accordance with common practice, these dimensions are recorded in inches (to the nearest half inch) and pounds respectively for height and weight.

The records discussed above form the principle series involved in the present analysis. Because of the rather limited data on infant length, however, an effort was made to augment the material from

whatever records could be secured from the files in the Biometric Laboratory at the University of Minnesota. In the data collected by the late Professor J. Arthur Harris were found two additional series secured through the courtesy of Dr. F. L. Adair, while on the staff of the University of Minnesota. They represented the beginnings of a proposed more extensive series, the abstracting of which was cut short by Professor Harris's death. The records embraced 912 private cases and 425 Minneapolis General Hospital cases, all of which were delivered by Dr. Adair or during his service. We feel particularly obliged to Dr. Adair for allowing us the use of these data, which were transcribed by his own assistants for a study with which we initially had no connection.

The various series of data will henceforth be referred to under the following key:

M_1 —Minneapolis General Hospital cases of Dr. Adair (1927-'29).
M_2 —Minneapolis General Hospital cases of Dr. Urner ((1930-'32).
M —Minneapolis General Hospital cases in all, i.e. M_1 plus M_2.
MP—Minneapolis private cases of Dr. Adair.

The first two series are of essentially the same character and differ only in the period covered. It will be shown later that they may be combined because of lack of real differentiation, and the symbol M without subscript will refer to the total series. The private cases must, from certain points of view, be handled separately because of significant differences in the maternal and offspring characters.

The well kown differentiation of the sexes with respect to weight and length at birth leads us to analyze the sexes separately throughout the following discussion.

RESULTS

a. The Univariate Distributions for Length and Weight.

The means and standard deviations (meaning type and deviation from type respectively) for length of the child at birth are given in Table 1. The corresponding statistics for stature of mother are presented in Table 2.

With respect to the two Minneapolis General Hospital series, M_1 and M_2, it will be observed that the differences between them with respect to both mean and variability of the child's length are quite

TABLE 1

Statistics for Length of Child (in cm.)

SERIES	N	MEAN, $\overline{h}$	ST. DEV., σ_h
		Male Infants	
M_1	206	50.8350 ± .1722*	2.4716 ± .1217*
M_2	229	51.0262 ± .1836	2.7785 ± .1298
MP	447	50.8904 ± .1396	2.9514 ± .0987
M	435	50.9356 ± .1265	2.6393 ± .0894
		Female Infants	
M_1	212	49.7358 ± .1718	2.5021 ± .1215
M_2	215	49.8000 ± .1737	2.5467 ± .1228
MP	458	50.1376 ± .1673	3.5801 ± .1183
M	427	49.7681 ± .1222	2.5248 ± .0864

*Standard errors are used throughout.

TABLE 2

Statistics for Height of Mother (in inches)

SERIES	N	$\overline{H}$	σ_H
		Mothers of Boys	
M_1	206	63.0583 ± .1736	2.4920 ± .1228
M_2	229	62.6725 ± .1629	2.4659 ± .1152
MP	447	64.1365 ± .1116	2.3586 ± .0789
M	435	62.8552 ± .1192	2.4858 ± .0843
		Mothers of Girls	
M_1	212	62.6981 ± .1930	2.8106 ± .1365
M_2	215	62.8047 ± .1533	2.2473 ± .1084
MP	458	64.2402 ± .1031	2.2064 ± .0729
M	427	62.7518 ± .1231	2.5432 ± .0870

small. The differences are statistically insignificant in all cases, and the combination of the two series is therefore quite justifiable.

Comparing now the *M* and *MP* series, it is found that while the slight differences in mean length of child are not significant, there is a significantly greater variability in the length of the infants of the private patient series. The statistics demonstrating this are given in Table 3. The greater rather than less variability in this connection was not anticipated. It forms a basis for speculative thought which is outside the scope of this enquiry and will not be traced here.

TABLE 3

Comparison of Statistics for Length of Infant in the M and MP Series

	BOYS		GIRLS	
	$\overline{h}$	σ_h	$\overline{h}$	σ_h
MP-M	$-.0452 \pm .1884$	$.3121 \pm .1332$	$.3695 \pm .2072$	$1.0553 \pm .1465$
Prob.*	.810	.019	.075	$<.001$ (5.6×10^{-13})

*Probabilty that the observed difference would be exceeded through errors of random sampling only.

TABLE 4

Comparison of Statistics for Height of Mother in the M and MP Series

	$\overline{H}$	σ_H
MP - M	$1.3850 \pm .1144$	$-.2315 \pm .0809$
Prob.*	$<.001$ (5.1×10^{-34})	.004

*See footnote to Table 3.

As for the infants, so the mothers in series M_1 and M_2 do not differ significantly in mean or variability of stature. When these two series are combined as series *M* and compared with the series *MP*, significant differences again appear. It will be seen from Table 4 that the private patients were generally taller and showed less variability in stature than the public hospital patients. This is not surprising at all, for one may expect the *M* series to embrace a more heterogeneous racial mixture of a somewhat more diverse and on the average lower socio-economic status than those in the *MP* series.

Statistics for weight at birth of the infants are given in Table 5, and those for weight of mothers at discharge from the hospital appear in Table 6. It will be observed that the infants born at the Minneapolis General Hospital during the period 1930-32 were somewhat lighter than those born there during 1927-'29, but in neither sex is the difference large enough to be regarded as statisically significant. From Table 7, it will be seen that the infants of series *MP* are somewhat lighter than those of the combined series, *M*, but the differences are very small and quite insignificant. While the variabilities of the *M* and *MP* series of infants do not differ significantly for girls, such is not the case with the boys. Bearing in mind the much larger number of cases available for weight, it will be seen that, in general, the *M* and *MP* series are much more alike for weight of the infants than they were for length.

TABLE 5

Statistics for Weight of Child (in cms.)

SERIES	*N*	$\overline{w}$	σ_w
		Male Infants	
M_1	206	3511.65 ± 43.01	617.35 ± 30.41
M_2	1789	3465.18 ± 13.30	562.81 ± 9.41
MP	447	3463.09 ± 23.57	498.28 ± 16.67
M	1995	3469.98 ± 12.74	568.86 ± 9.01
		Female Infants	
M_1	212	3358.49 ± 35.26	513.38 ± 24.93
M_2	1721	3327.25 ± 12.97	538.15 ± 9.18
MP	458	3291.27 ± 24.31	520.30 ± 17.19
M	1933	3330.68 ± 12.18	535.58 ± 8.61

TABLE 6

Statistics for Weight of Mother (in lbs.)

SERIES	N	$\overline{W}$	σ_w
		Mothers of Boys	
M_1	206	134.660 ± 1.661	23.840 ± 1.174
M_2	1789	135.006 ± .556	23.504 ± .393
MP	447	136.253 ± .942	19.916 ± .666
M	1995	134.970 ± .527	23.539 ± .373
		Mothers of Girls	
M_1	212	134.340 ± 1.529	22.266 ± 1.081
M_2	1721	133.797 ± .578	23.965 ± .409
MP	458	136.332 ± .937	20.059 ± .663
M	1933	133.857 ± .541	23.785 ± .383

Turning now to the statistics for the mothers (Table 6), one may summarize the situation briefly by stating that series M_1 and M_2 differ insignificantly and may be combined. Also, mothers of boys do not differ significantly from mothers of girls with respect to weight, and accordingly all mothers may be compared directly for series M and MP. This is done in Table 8, wherein it will be observed that the private cases averaged nearly two pounds heavier at discharge and were considerably less variable. Both differences are taken as significant, and, as in the case of length of mother, we note that the private cases are generally larger and more homogeneous a group than the public hospital patients.

There is nothing to be gained here by speculative exploration of plausible reasons for the observed differences between the public and private patients series. The facts are: (1) the private case mothers were of larger and less variable build than the patients in the obstetric ward of the city public hospital; (2) the infants of these two series do not differ significantly in mean length or weight, but (3) they tend to be more variable in these characters for the private cases, particularly with respect to length. The important point is to recognize the existence of this heterogeneity and eliminate its influence from the measures of correlation by treating the two series separately.

TABLE 7

Comparison of Statistics for Weight of Infant in the M and MP Series

	BOYS		GIRLS	
	$\overline{w}$	σ_w	$\overline{w}$	σ_w
MP-M	−6.89 ± 26.79	−70.58 ± 18.95	−39.41 ± 27.19	−15.28 ± 19.23
Prob.*	.795	<.001 (2×10^{-4})	.147	.430

TABLE 8

Comparison of Statistics for Weight of Mother in the M and MP Series

	$\overline{W}$	σ_W
MP-M	1.871 ± .764	−3.678 ± .541
Prob.*	.014	<.001 (10^{-38})

*See footnote to Table 3.

b. The Correlation for Length

The correlations between stature of mother and length of infant son and daughter are given in Table 9. The corresponding values of Fisher's logarithmic transformation for r,

$$z_r = \tfrac{1}{2} \log_e \frac{1+r}{1-r}$$

are also tabulated for each series. All of the comparisons between the correlations for the various groups will be based on comparison of z_r values, since the form of distribution is constant and "normal" and the standard error may be taken as depending only on the size of the

sample considered. Thus the significance of the correlations and their differences may be tested more precisely in terms of z_r.

TABLE 9

Correlation Coefficients (r) for Stature of Mother and Length of Child

SERIES	N	r	$z_r \pm S.E.$	P*
		Male Infants		
M	435	.1419	$.1428 \pm .0481$	$<.001$ (3×10^{-4})
MP	447	.1159	$.1164 \pm .0475$	.014
		Female Infants		
M	427	.2000	$.2027 \pm .0486$	$<.001$ (3×10^{-4})
MP	458	.1531	$.1543 \pm .0469$	$<.001$ (10^{-4})

*Probability that the observed r would arise by random sampling from an uncorrelated supply.

The correlations are all positive, of about the same magnitude, and although not of a high order are significantly greater than zero. Apparently the two series are not differentiated with respect to the intensity of correlation between stature of the mother and length of child of either sex, although the values observed for series *MP* are a trifle lower than those for the *M* series. The correlations for girls are somewhat higher than those for boys, but neither of the differences observed is statistically significant. Accumulating the coefficients into a single value using the logarithmic transformation technique, one finds that

$$\bar{z}_r = +.1536, \text{ with S.E. of } .0239$$

and

$$\bar{r} = +.1524$$

Since $\bar{z}_r$ is over 6 times the appropriate standard error, there can be no doubt that there is in general a real and positive correlation between the

length of infants at birth and the stature of their mothers. The data under investigation indicate this correlation to be of the order of +.15 if the regression be rectilinear, or greater if curvilinearity exists in the trend of average relationship. It is desirable, therefore, to pass at once to consideration of the form of regression.

In the upper and lower panels of Figure 1, the empirical regressions are given for boy's and girl's length respectively on maternal stature. Each solid dot represents an average based on a class frequency of at least 10 cases, while the circles denote smaller frequencies. The latter

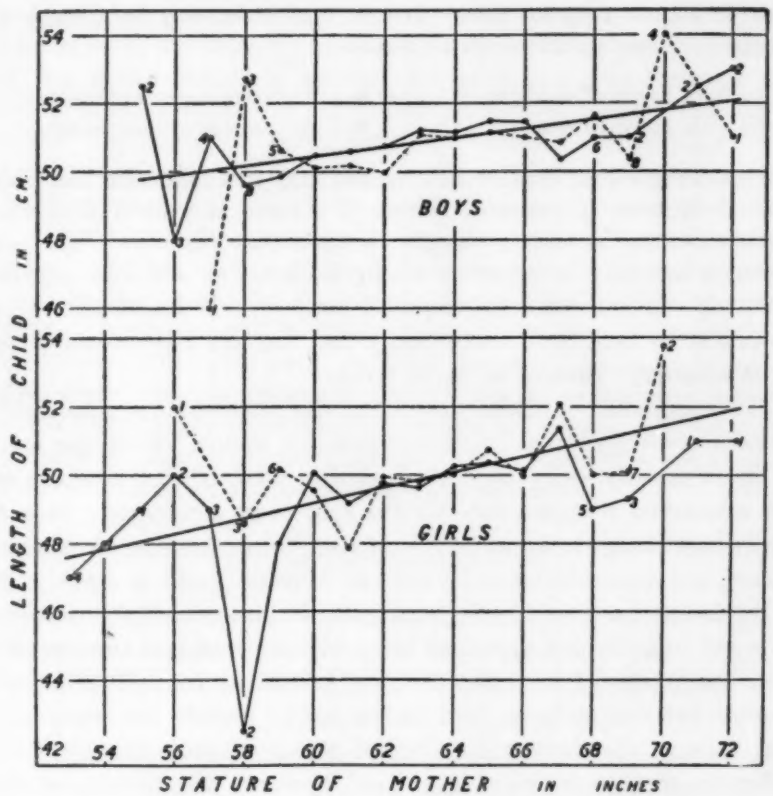


FIG. 1. EMPIRICAL AND THEORETICAL REGRESSION LINES FOR THE DEPENDENCE OF LENGTH OF CHILD ON STATURE OF MOTHER.

are given in juxta-position. The solid and broken connecting lines designate the *M* and *MP* series respectively.

The statistical worker experienced in regression problems will recognize at once that, for both series and both sexes, the trend of these lines joining observed averages is essentially of a rectilinear character. It will also be observed that there is little, if any, difference between these trends for the two series within any one sex. Statistical tests show that the separate theoretical rectilinear regression lines for each series within each sex are not significantly different, and for simplicity they have not been individually included. We have, however, fitted the rectilinear regression in each sex for *all* available cases. These are given in Figure 1 by the heavy straight lines traversing the groups of averages. Their equations are as follows:

For boys, length of child (in cm.) $= 42.28 + .14$ stature of mother (in inches)
For girls, length of child (in cm.) $= 35.78 + .22$ stature of mother (in inches)

From the regression coefficients (.14 and .22) it would seem that each inch of increase in maternal stature is accompanied by a somewhat greater average increase in length for girls than for boys. The two values, which may be expresed wholly in inches as .088 and .054 respectively, do not differ significantly, however. It would seem very reasonable to hypothecate accordingly that the two lines would prove to be essentially parallel in larger series.

Two important statistics flow from this study of the relationship between new-born infant length and maternal stature. First, the value of approximately +.15 on the correlation scale for the intensity of this association is appropriate for the 1767 cases considered. Second, the average change in infant length is a rectilinear function of maternal stature, and approximates to .07 units of increase $[(.088 + .052) \div 2]$ in the former for 1 unit in the latter. To the obstetrician, this relationship will probably not appeal as being of much practical consequence, since the infants of the tallest mothers considered (of 6 feet stature) average but one and one-third inches longer (within the same sex) than those of the shortest mothers (of 4 feet 5 inches stature). This difference does assume more importance, however, when it is noted that the mean difference between the boys and girls in length is only 0.4 inches, a value which is somewhat larger than that usually found (*e.g.* see Harris, Treloar, and Gunstad, 1936).

On the other hand, students of anthropometry and of human physical heredity will find in these figures much of considerable interest. It has been indicated before that the correlation between maternal and *adult* offspring statures is of the general order of +.50, whereas the coefficient falls here to +.15 when the offspring are measured at birth. Apparently, the physical expression of intensity of heredity is less at birth than at maturity. Considering the very great uniformity of the conditions surrounding pre-natal life, together with the single bloodstream of the maternal organism and foetus, this might prove to be a surprising result to many. It is certainly provocative of thoughtful consideration of the relative importance of the forces at play. On the one hand one asks: To what extent is the development of stature in life after birth determined by nutrition and environment? There surely would, in general, be much similarity between these forces as they effect any one generation and its progeny forming the next. On the other hand, one must consider the problem of the extent to which birth is incited by the dependent organism reaching a critical stage of development. After all, there are many grounds for considering that the instigation of the processes of delivery is a physiological reaction, which, within a time interval of several weeks, at least, is unrelated to the stage of development of the rapidily growing child. One is disposed to believe that we face here a situation where conjecture, although resting on quite acceptable bases of knowledge, may nevertheless be quite misleading. We are exploring a field here where considerably more quantitative data are needed, and the authors propose to gather such along several lines in which it is felt it may in time be secured in extensive series of great value.

c. The Correlation for Weight

The coefficients of correlation between post-partum weight of mother and weight at birth of the child, given in Table 10, are appreciably higher than those given in Table 9 for the respective length measurements. For the *M* series they are of the order of +.31, while the correlation coefficients for the *MP* series average approximately +.22. It will be observed from the empirical regression lines in Figure 2, however, that the trend appears to be curvilinear in form rather than rectilinear. We have therefore calculated the correlation ratios in order to examine this point critically. These are given under

the symbol E in Table 10, the asterisked values indicating a significant* increase over the corresponding value for r. Such increases as are significant establish the point that regression is *not* rectilinear for the data considered. Where curvilinearity is not in this manner demonstrated, it may of course exist but fail to be proven for such series alone because of insufficient data. Curvilinearity may be taken as proven without doubt for the larger M series. Since the MP series shows essentially similar trends in Figure 2 to the M series, we feel that real curvilinearity exists in both and that the correlation ratios should be accepted as the more dependable measures of the actual intensity of association. Thus the intensity of correlation becomes of the general order of +.30 for the entire material available for the weight relationship (4833 cases). It is of considerabe interest to note

TABLE 10

Correlation Coefficients (r) and Correlation Ratios (E) for the Association of Weight of Child with Weight of Mother

SERIES	N	r	E	$(E-r)$
		Male Infants		
M	1995	+.3103	.3439*	.0336*
MP	447	+.2454	.2981	.0527
		Female Infants		
M	1933	+.3079	.3574*	.0495*
MP	458	+.1850	.2780	.0930

that this value is double that already determined for the length relationship; in other words, the new-born child is patterned after its mother more with respect to weight than with respect to length. In neither case, however, is the correlation high.

The problem of selecting the appropriate curved regression line for the average dependence of child's weight on that of the mother is rendered difficult by the low frequencies of cases in the higher maternal

*Woo's tables were used for this test.

weight classes. In Figure 2 we present the empirical regression lines with the solid dots indicating average weight of child based on 10 or more (generally 100 or more) cases in the class. The circles this time indicate averages of 10 or more cases (usually just over 10) *in classes which have been made of wider range* to secure this minimum frequency. Study of these empirical regressions will show that there is a distinct increase in child's weight as the maternal weight increases from the lowest values, that this rate of increase *falls* as the independent variable increases until, in the region of say 190 lbs. maternal weight, the regression slope has vanished and may indeed become negative thereafter. Thus the upper region becomes critical in determining the curve form. Many more cases are needed in this region than are available to us in order to portray dependably the type of this most interesting curvature. In view of present limitations we have decided to limit this investigation to a study of the graduation provided by the polynomial,

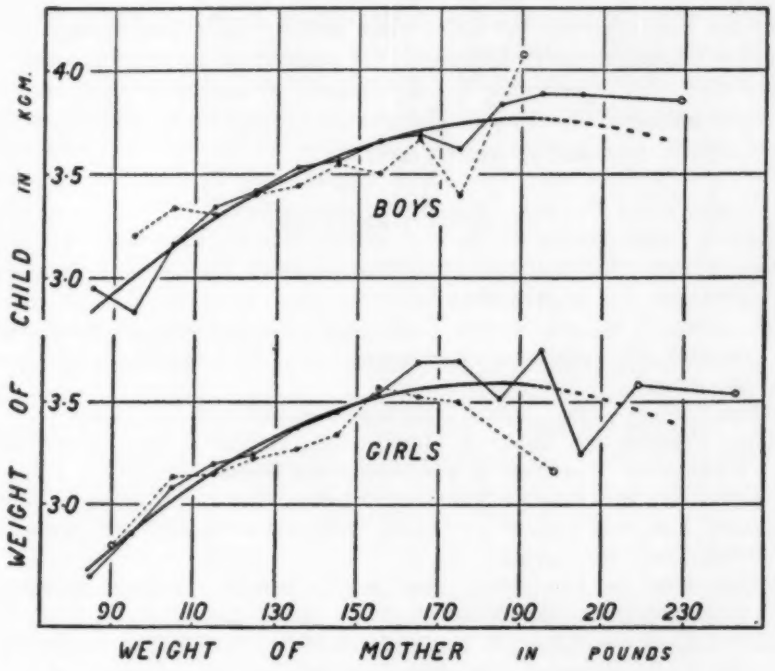


FIG. 2. EMPIRICAL AND THEORETICAL REGRESSION LINES FOR THE DEPENDENCE OF WEIGHT OF CHILD ON WEIGHT OF MOTHER.

$$w = a + bW + cW^2,$$
where w = child's weight,
and W = mother's weight,

i.e. by a parabolic regression. For simplicity we have again combined the *M* and *MP* series for this purpose, and the smooth curves given in Figure 2 represent the graduations so secured. Assuming that the *form* of regression is not sexually differentiated, these curves would appear to provide fairly satisfactory graduations as a first approximation within the regions of fairly high frequency (90 to 180 lbs. maternal weight). In the upper regions where the curves are given in broken-line form, we have considerable doubt that they portray the true relationship. A horizontal line in this region would seem to accord as well with the observations as a falling curve.

The interesting feature of this regression study to the authors is the very definite diminution of the regression slope as the maternal weight increases above the average zone of 130-135 lbs. Below this zone the slope changes but little, while above it falls steadily, and *may* possibly become negative ultimately. A temptation exists to conjecture from these data concerning the significance of excessive adult weight as representing a condition of disturbance against the impact of which the foetus is defended in nature's economy.

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ESTIMATES OF INTRA-INDIVIDUAL AND INTER-INDIVIDUAL VARIATION OF THE ERYTHROCYTE AND LEUKOCYTE COUNTS IN MAN*

BY E. MORTON JELLINEK

(Of the Memorial Foundation for Neuro-Endocrine Research and the Research Service of the Worcester State Hospital)

THE knowledge of the variation of physiological functions in humans is largely limited to that variation which is observed to obtain *between* different individuals. The fluctuations *within* individuals, especially within healthy individuals, have been studied only infrequently, mainly because of the difficulty in securing sufficiently large series of observations on the same persons. Fisher's "Analysis of Variance" affords us, however, a means of estimating the within-individual variance (s^2) when a small number of repeated observations is available for each member of a group. Physiology is only beginning to avail itself of this method. At present physiologists are generally inclined to use the mean individual range to characterize the intra-individual variation. At the Worcester State Hospital a large body of physiological data has been collected on samples of schizophrenic patients and normal control subjects (1). For some variables, such as fasting oxygen consumption rate, blood pressure, and pulse rate, the data gathered are so extensive that frequency distributions for each experimental subject are available. Most of the physiological variables, however, have been tested but two to six times per subject, and for these variables the investigation narrows down to estimates of the average intra-individual variance.

In spite of the extensive literature on the variation of blood counts, the aspect of intra-individual variation has received little attention. Recently Garrey and Bryan (2) have reviewed the literature on the variation of leukocytes. Two facts are outstanding in this review:

*This study is based on data collected by various members of the Worcester Research staff, to whom the writer is greatly indebted.

(a) Most of the investigations refer to variation under differing conditions, though there are some studies relating to variation for a given condition, such as basality. (b) Though a large number of observations have been secured for each of a small number of subjects, there is no suitable material for an estimate of the intra-individual variation, or else it has not been utilized for this purpose. Thus it seems justified to evaluate here estimates from the data collected at the Worcester State Hospital.

Although the larger porportion of the data comes from a sample of schizophrenic patients, the findings agree so well with those on normal controls that we do not hesitate to make the general statement that these estimates pertain to the variation of erythrocyte and leukocyte counts in man. It should be noted that the schizophrenic patients admitted to the research service are selected on the basis of absence of all complicating physical diseases.

DESCRIPTION OF THE SAMPLES

All observations were made under basal conditions, that is, in a state of post-absorption and physical relaxation. Under such conditions physiological variables should display a minimal variation. The various samples utilized here consisted of a varying number of male schizophrenic patients and male normal controls. The age range for both groups was from 20 to 45 years. Over this range age has little or no effect on most physiological variables. Patients and controls were subjected to a strict physical examination and only those in good physical health were included in the samples.

The samples utilized here emanate from two orientation studies with extensive testing schedules. The first study was carried out in the years 1931 and 1932 and is referred to in reports on the Worcester project as the "Seven Months Study". The blood morphology was one of 70 quantitative variables studied. Blood counts were made twice during the first month, about 7 days apart. In the fourth and seventh month again two readings 7 days apart were obtained. Although 93 patients were included in this study, only slightly more than half of the patients yielded readings for all of the six tests of this "Seven Months Study". We are giving here estimates of variation based on: (a) Two successive readings 7 days apart for 93 patients (b) Two successive readings 7 days apart for the first, fourth, and seventh month separately for 51 and 54 patients for erythrocytes and leukocytes res-

pectively. (c) All the six readings during the seven months for 51 and 54 patients respectively. (d) Lastly, two readings seven months apart, namely, the first reading of the first month and the first reading of the seventh month for the same number of patients as above. These samples permit us to evaluate the consistency of the estimates of the variance, as well as to measure the influence of different time intervals on the magnitude of the variance. In this first study the normal controls were recruited from the hospital staff, and had but one count each. Thus we cannot estimate the intra-individual variance for this sample of normal controls.

The second study took place during 1934 and 1935. Each of 30 patients, not contained in the above "Seven Months" sample, was tested simultaneously with a normal control during a period of six weeks. The normal controls were hired for this purpose, and lived on the wards under the same conditions as the patients. In this series the blood morphology was determined only three times. Samples of two readings 7 days apart are utilized here both for patients and normal controls. A further check on consistency, as well as an estimate for normal subjects, is obtained.

THE METHOD OF ANALYSIS

The values tabulated in Tables 1 and 2 refer to analyses of variance performed on the samples described above. The analysis of variance furnishes various estimates of variances according to subsets into which the sample may be divided. In our samples each individual constitutes a subset, and thus the estimate of the average within-subset variance represents the average within-individual variance. The between-subset variance is an estimate of the variance obtaining between the means of individuals. Strictly, the evaluation of variation should be in terms of variance for reasons which need not be discussed here. Since, however, the variance is in terms of the squares of the original units of measurement, the figures become rather bewildering, and for that reason the corresponding standard deviations too are tabulated. It is not strictly true to say the "corresponding standard deviations" since the square root of the mean variance does not coincide with the mean of the standard deviations. For all practical purposes the difference is negligible.

Another value tabulated here is Snedecor's "F" criterion which is the ratio of the larger variance to the smaller variance. This criterion

is used for the testing of the significance of the difference between two variances. The corresponding P values of this criterion are taken from Snedecor's Tables (3) which give the 5 per cent and 1 per cent values of "F".

Since it might be of interest to compare the intra-individual variation in the erythrocyte count with that in the leukocyte count, a comparable measure of variation is furnished in the form of the intra-class correlation coefficient for which our notation is r'. Intra-class correlation is a special case of the analysis of variance when the number of readings for each subset is equal. It can only be used when the nature of the data is such that x and y can be inter-changed without affecting the relation. Such a case arises when we wish to correlate the first and second, or any other repeated readings of a variable, on the same individuals. If no systematic shift has taken place between repeated readings, then it becomes irrelevant which member of the pair is the first and which the second or third reading. Then we can take deviations from the common mean of the replicated observations and compute the intra-class correlation coefficient from

$$r' = \frac{1}{n's^2} S \left\{ (x - \bar{x})\ (x' - \bar{x}) \right\}$$

(r' is, however, generally directly obtained from the analysis of variance.) It is evident from this definition that a correlation of $+1$ will be obtained only if the members of the various subsets are identical. This property is of the greatest importance when consistency of a physiological function is to be measured in terms of self-correlation, since a high degree of the ordinary inter-class correlation does not imply closeness to identity but only constancy of relation. Thus, whether or not the *level* has been maintained in repeated readings could not be determined through the inter-class correlation alone but would require knowledge of the regression equation as well.

DISCUSSION OF THE TABULATED ESTIMATES

Table 1 lists estimates of variances pertaining to the erythrocyte count. The results may be summarized as follows:

(1) Samples A to D give evidence of the consistency of the estimates of the intra-individual variance when time intervals between repetitions are equal. Since, however, these samples are not inde-

TABLE 1

Estimates of the Intra-Individual and Inter-Individual Variation of the Erythrocyte Count. Erythrocytes in Units of Ten Thousand

SAMPLE		DEGREES OF FREEDOM	SUM OF SQUARES	VARIANCE	STANDARD DEVIATION	"F"	P	INTRA-CLASS CORRELATION
93 Schizophrenics 2 readings 1 week apart A	Within Individuals	93	56452	607	24.6	6.3	<.01	.72
	Between Individuals	92	352495	3831	61.9			
51 Schizophrenics 2 readings 1 week apart B	Within Individuals	51	27877	547	23.4	4.8	<.01	.65
	Between Individuals	50	131616	2632	50.3			
Same Sample 3 months later 2 readings 1 week apart C	Within Individuals	51	30642	601	24.5	5.8	<.01	.70
	Between Individuals	50	175577	3512	59.2			
Same Sample 7 months later 2 readings 1 week apart D	Within Individuals	51	30733	603	24.5	6.5	<.01	.73
	Between Individuals	50	197618	3952	62.7			

TABLE 1 (*con't*)

SAMPLE		DEGREES OF FREEDOM	SUM OF SQUARES	VARIANCE	STANDARD DEVIATION	"F"	P	INTRA-CLASS CORRELATION
Same Sample All the six readings of the 7 months	Within Individuals	255	332265	1303	36.1			.37
	E					4.5	<.01	
	Between Individuals	50	295750	5915	76.9			
Same Sample 1st reading of 1st month and 1st reading of 7th month	Within Individuals	51	55247	1083	32.9			.43
	F					2.6	<.01	
	Between Individuals	50	139920	2798	52.9			
30 Schizophrenics not contained in above samples 2 readings 1 week apart	Within Individuals	30	15815	527	22.9			.58
	G					4.0	<.01	
	Between Individuals	29	61152	2109	45.9			
30 Normal Controls. 2 readings 1 week apart	Within Individuals	30	21818	727	26.9			.45
	H					2.8	<.01	
	Between Individuals	29	58095	2003	44.7			

pendent, being constituted of the same individuals, one may be inclined to ascribe the consistency to the intra-class relations. This argument loses weight, however, when we compare the above samples with samples G and H which are entirely independent samples of patients and normal controls respectively, but for which the time intervals are the same as in samples A to D. Estimates derived from repetitions on the same sample of patients, as well as estimates from an independent sample of patients and a sample of normal controls seem to justify the statement that the estimate of intra-individual variance of erythrocyte count for comparable time intervals is reproduceable. In terms of the more familar concept of standard deviation, we may say that under basal conditions, the average man, between the ages of 20 to 45 years, displays for an interval of one or two weeks a variation of approximately 240,000 erythrocytes, while the variation between individuals is approximately twice as great. It should be noted, however, that in terms of the more important concept of variance the inter-individual variability for this short time interval is about five times as great as that within individuals.

(2) Comparing samples E and F which contain repetitions of blood counts over a period of seven months, with samples A to D in which repetitions are one week apart, it appears that the intra-individual variance of the erythrocytes increases as the time interval between observations is lengthened. In terms of standard deviation, the average man with the qualifications stated above displays for such long intervals as seven months a variation of approximately 350,000 erythrocytes. The inter-individual variation does not seem to be perceptibly affected by the longer time interval. Comparing the intra-individual variances of samples E and F with the corresponding estimates in samples A to D we find that the differences are significant in all instances. The P values for the corresponding "F" criteria are all approximately .02.

The phenomenon of the increase in intra-individual variation with increasing time intervals is quite common to the variation which we have found in other physiological variables. It may be produced by seasonal trends or by slowly developing physiological processes. In some instances, however, it may be due to an artifact. It is a well-known fact that the technician's skill increases with practice and thus two determinations near each other with respect to time would show less technical variation than two determinations far apart. In our case, however, the question of a technical artifact may be disregarded entirely,

TABLE 2

Estimates of the Intra-Individual and Inter-Individual Variation of the Leukocyte Count. Leukocytes in Units of One Hundred

SAMPLE			DEGREES OF FREEDOM	SUM OF SQUARES	VARIANCE	STANDARD DEVIATION	"F"	P	INTRA-CLASS CORRELATION
93 Schizophrenics 2 readings 1 week apart	A	Within Individuals	93	698,207	7,508	27.4	2.9	<.01	.48
		Between Individuals	92	2,028,228	21,809	46.7			
54 Schizophrenics 2 readings 1 week apart	B	Within Individuals	54	307,113	5,687	23.8	2.9	<.01	.48
		Between Individuals	53	882,219	16,646	40.8			
Same Sample 3 months later 2 readings 1 week apart	C	Within Individuals	54	382,357	7,081	26.6	1.6	>.01	.23
		Between Individuals	53	609,138	11,493	33.9			
Same Sample 7 months later 2 readings 1 week apart	D	Within Individuals	54	442,199	8,189	28.6	3.1	<.01	.51
		Between Individuals	53	1,365,834	25,770	50.8			

TABLE 2 (*con't*)

SAMPLE			DEGREES OF FREEDOM	SUM OF SQUARES	VARIANCE	STANDARD DEVIATION	"F"	P	INTRA-CLASS CORRELATION
Same Sample All the six readings of the 7 months	E	Within Individuals	270	2,582,071	9,563	30.9	2.9	<.01	.24
		Between Individuals	53	1,497,959	28,263	53.2			
Same Sample 1st reading of 1st month and 1st reading of 7th month	F	Within Individuals	54	764,018	14,149	37.6	1.4	>.05	.16
		Between Individuals	53	1,064,257	20,080	44.8			
30 Schizophrenics not contained in above samples 2 readings 1 week apart	G	Within Individuals	30	91,875	3,063	17.5	2.8	<.01	.46
		Between Individuals	29	247,266	8,526	29.2			
30 Normal Controls. 2 readings 1 week apart	H	Within Individuals	30	92,928	3,098	17.6	1.1	>.05	.06
		Between Individuals	29	103,591	3,572	18.9			

since at the time of these studies our technicians had had many years' experience in the counting of blood cells, and presumably had reached a plateau of practice effect. We have definite evidence of increasing variance with increase in time in some cases where the question of technical artifact is entirely removed. Such is the case in the variance of urinary volume, where the operation of any technical factor is improbable.

(3) From the tabulated "*F*" and *P* values it can be seen that in all cases the individual is significantly more homogeneous than the group in respect to erythrocyte count.

In Table 2, which refers to leukocyte variations, the following items may be noted:

(1) As far as samples A to D are concerned, there is great consistency in the estimates of the intra-individual variance of leukocytes. Examining samples G and H, however, which are equal to the above with respect to time intervals, we find a significant reduction in the intra-individual variance. The following explanation of this discrepancy is suggested: At the time of the "Seven Months Study" the only stipulations in connection with the taking of blood samples from the ear, were post-absorptional state and absence of exercise. In our later studies, (samples G and H) we added to these stipulations postural basality, following Garrey's suggestions (4). Garrey has shown that change of posture causes fluctuations in the leukocyte counts. This statement has given rise to a vigorous controversy. The reduction in variance which has been obtained here under change of technique seems to bear out Garrey's contention.

On the whole, it may be said that under basal conditions, but in the absence of postural standardization, and for a short interval such as one or two weeks, the average intra-individual variation of leukocytes in terms of standard deviation is approximately 2700 cells. Keeping posture constant (reclining position) this intra-individual variation may be reduced to 1700 cells. All these statements must be qualified as applying to males in the age range 20 to 45 years, and in the absence of physical diseases.

(2) As in the case of erythrocytes, we find a significant increase in intra-individual variance of leukocytes with increasing time intervals between observations. For observations extending over a period of seven months the intra-individual variation in terms of standard deviations may rise to 3700 cells.

(3) Although the intra-individual variance in leukocyte count is high, generally it is significantly smaller than the between-individual variance. In samples F and H there may be no significant difference between the intra-individual and the inter-individual variances.

(4) Comparing the intra-class correlation coefficients of Tables 1 and 2 it appears that the individual is much less consistent with respect to leukocyte count than with respect to erythrocyte count.

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SOME USES OF THE STANDARD DEVIATION IN ANTHROPOMETRY

BY W. W. HOWELLS

American Museum of Natural History

THE standard deviation (or "sigma") is a constant which describes the dispersion of the individual cases about the mean in a normal frequency distribution. It furnishes a check upon the process of sampling, and it also gives a rough clue to the relative homogeneity of the series. With respect to the latter function, however, it should be remarked that the number of cases in any sample has a considerable influence on the behavior of sigma. By and large, when the sample is small the sigma is erratic and may be either very low or very high (more often the former), since the extreme cases in the array affect it strongly; as the sample enlarges the sigma becomes less variable, and increases in absolute size, considerably at first, and then very slightly as the sample becomes very large. This behavior follows from the nature of the standard deviation itself.[1] However, the author wished to observe actual data, and assembled the sigmas, in a number of anthropometric characters, of over 150 samples of different racial stocks the world over.[2] The tabulation of these bore out the above observations, and indicated that a series of 50 was the smallest of which the sigmas could be accepted as affording

[1]This may be demonstrated algebraically:

Mean sigma of samples of $n = \sqrt{\frac{n-1}{n}}$ (sigma of universe)

Standard deviation of the sigmas of samples of $n =$ (sigma of universe) $/\sqrt{2n}$

Therefore as n increases the mean sigma must increase and the standard deviation of the sigmas must decrease.

For the above expressions I am indebted to Dr. Raymond Pearl.

[2]Series, from well-known sources, of adult males only, chosen to represent all areas as equitably as possible; for this reason the many Scandinavian series published by Bryn and Schreiner and by Lundborg and Linders were not used. Possibly hybrid or distinctly heterogeneous groups were also excluded.

any information as to group variability. Series numbering from 10 to 25 cases gave extremely erratic sigmas, but as the number increased beyond 50 there was no appreciable diminution in variability, and only a very slight increase in the average size of sigma.

MEAN SIGMAS

In Table 1 are presented the mean standard deviations for each character recorded, calculated without weighting from the sigmas of all the available series of 50 or more cases. (The table also gives in each character the standard deviation of these sigmas and a range on either side of the mean equalling three times the standard deviation, within which the sigmas of any sample of 50 or more may be expected to fall. A sigma approaching or exceeding these limits is apt to be due to a mistake in calculation or to improper sampling).

TABLE 1

Mean Sigmas

	M	S.D.	RANGE, 6×S.D.
Stature (79)	5.81	.66	3.83- 7.79
Head length (78)	6.23	.56	4.55- 7.91
Head breadth (77)	5.24	.59	3.47- 7.01
Minimum frontal (27)	4.88	.72	2.72- 7.04
Bizygomatic (56)	5.28	.67	3.27- 7.29
Bigonial (34)	5.79	.50	4.29- 7.29
Face height (52)	6.43	.61	4.60- 8.26
Upper face height (26)	4.66	.35	3.61- 5.71
Nose height (66)	3.77	.49	2.30- 5.24
Nose breadth (67)	2.92	.38	1.78- 4.06
Cephalic index (76)	3.39	.56	1.71- 5.07
Cephalo-facial index (23)...	3.51	.59	1.74- 5.28
Facial index (49)	5.09	.57	3.38- 6.80
Upper facial index (26)....	3.63	.33	2.64- 4.62
Nasal index (67)	7.82	1.38	3.68-11.96

This unrefined treatment seems the most suitable under the circumstances, and the mean sigmas are therefore offered for want of something more precise, since a perfect universe is not attainable, in

the hope that they may be of some help in questions of the relative variability of populations and of anthropometric characters.

In the first place, the extent of the differences in variability existing between different measurements and indices may be clearly seen. It is plain that the scale of magnitude is not the only differential; that there is an inherently greater variation in some characters than in others. The mean sigma for face height, for example is considerably larger than that for head breadth. Another factor seems to be reflected in these differences: namely, that certain measurements on the living may be determined more precisely than others. The extent of such an influence might be found by comparing these mean sigmas with comparable ones derived from cranial series.

SIGMA RATIO

A more specific use for the mean sigmas lies in applying them as a standard to individual sigmas. It might in this way be substituted for the Coefficient of Variation in comparing the variability of two anthropometric series as a whole. The Coefficient of Variation has a proper application in comparing the variabilities of different anthropometric criteria, as such, with one another, but it should not be used in comparing the mean variabilities of different human groups: the reason for this is that while it measures the variability of the sample, it is at the same time measuring the inherent variability of the character to which it applies, and the latter is a considerable differential which should be removed. In other words, the Coefficients of Variation in different measurements and indices are not comparable.

In an effort to correct this, the following device is offered, which is provisionally named the "sigma ratio". This is arrived at by dividing any individual sigma by the mean sigma for that character, giving a a ratio, or percentage of the mean sigma. Ideally, and on the average, this figure will approximate 100 (when expressed as a percentage) which may thus be taken as a norm. Therefore, for any given sample the mean sigma ratio for all available measurements and indices will constitute an index of the variability of that group relative to the general average, which is represented by 100.[8]

[8]During the preparation of this paper there has appeared a paper based on a somewhat similar approach: "An attempt to establish the relative variabilities of various populations," by G. M. Morant. The method used is as follows: large series are taken from the literature which are composed of a number of sub-series

For the purpose of gauging group variability this figure seems to compare favorably with the Coefficient of Variation on all grounds. In dividing the standard deviation by the mean sigma all characters are reduced to a common denominator as to the unit of measurement involved, as to relative dimensionality, and as to the variability of each character *per se,* while in dividing the standard deviation by the simple mean, the last factor is ignored. This may be stated concretely: the means of the nasal index and the cephalic index are on an average much the same size, but the normal variability of the nasal index is well over twice as great; and as a measure of purely biological variability the sigma ratio corrects this discrepancy while the Coefficient of Variation does not. It follows, therefore, that the mean sigma ratios of any number of series may be compared, even though they do not comprise the same characters throughout. A final advantage is that the sigma ratio has also an abstract point of reference, i.e., 100, so that a mean sigma ratio greater than 100 implies a group characterized by greater than average variability, and the reverse.

The sigma ratio has at least one shortcoming: it does not take into account the range of magnitude of a character. Tall or platyrrhine groups would tend to have slightly greater sigmas than short or leptorrhine ones, in stature or the nasal index, and the mean sigma being a constant figure makes no allowance for this. Such an error, though consistent, is probably almost negligible, however, and would tend in any case to be completely obscured by other irregularities of sigma, which have been shown to be considerable.

To translate the foregoing discussion into an example, the sigma ratio may be applied to the question of the variability of hybrid populations. There is general agreement that hybrid groups do not necessarily exhibit an increased variability in their measurable characteristics over the parent groups involved, but we may compare the sigma ratio

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(i.e., the Norwegian recruits), and from the weighted squared sigmas of the sub-series there are calculated sigmas for the total groups. Ratios between the sigmas (squared) of two racial groups are found in all characters, and the mean of these ratios is taken as the measure of relative variability between, for example, Norwegians and Central Asiatics.

This procedure is quite suitable for the purpose to which it is put. In the present case it was desired to have some absolute figure, however crude, which would be devoid of weighting in its caculation, so that comparisons would not be restricted to specific pairs of groups, and so that small and isolated groups as well as large, carefully chosen series might be subject to its application.

with other methods of demonstrating the fact. K. Wagner,[4] in a recent review of the well-known monographs on hybrid groups, employed the mean Coefficient of Variation as an instrument of comparison between hybrids and "pure" groups. Beside the faults of such a figure as already set forth may be mentioned the fact that it is largely controlled by the high Coefficient of the nasal index, as the author notes, or of such small-scale or erratic characters as nose breadth or "height of lip"; and therefore comparisons are possible at all only when the characters involved are identical in all groups.

The sigma ratio will, of course, not lead to conclusions differing from those already attained, but will merely render the material more com-

TABLE 2

Standard Deviations and Sigma Ratios of English-Tahitian Hybrids Compared with Parent Races.

	NORFOLK ISLAND (64)		SOCIETY ISLANDS (85)		BRITISH SCIENTISTS (84)	
	S. D.	Sigma Ratio	S. D.	Sigma Ratio	S. D.	Sigma Ratio
Stature	6.35	109.5	5.91	101.9	6.83	117.8
Head length	5.82	93.9	6.54	105.5	6.16	99.4
Head breadth	4.34	83.5	4.96	95.4	4.73	91.0
Minimum frontal	4.38	89.4	4.99	101.8	5.65	115.3
Bizygomatic	5.25	99.1	5.13	96.8	5.12	96.6
Bigonial	4.66	80.3	6.11	105.3	6.45	111.2
Face height	5.54	86.6	6.35	99.2	6.62	103.4
Nose height	3.26	85.8	3.51	92.4	3.84	101.1
Nose breadth	2.79	96.2	2.77	95.5	2.86	98.6
Cephalic index	2.86	84.1	3.84	112.9	2.61	76.8
Cephalo-facial index	2.81	80.3	2.65	75.7	2.67	76.3
Facial index	5.17	101.4	4.48	87.8	4.59	90.0
Nasal index	6.14	78.7	7.22	92.6	6.47	82.9
Mean		89.9		97.1		97.0

[4] 1932, "The variability of hybrid populations," *American Journal of Physical Anthropology*, vol. xvi, No. 3.

prehensible. Let us take Shapiro's English-Tahitian hybrids of Norfolk Island. When these were compared with Society Islanders and British scientists, taken to represent the parental groups, he found that the standard deviation of the hybrids was the lowest in a majority of characteristics, and explained this by "long continued and extensive inbreeding among the islanders." In Table 2 are noted for the three groups named the sigmas and sigma ratios of those measurements and indices for which we have mean sigmas.[5]

It is interesting to see that the mean sigma ratios of the Society Islanders (97.1) and of the English (97.0) are close to the average of 100, and the markedly lower figure for Norfolk Islanders lends force to Shapiro's explanation. For further comparison we may take the first ten provincial groups given by Bryn and Schreiner in their work on Norwegian recruits. The mean sigma ratio, for eight characters, ranges between 97.0 and 105.8 with a mean of 101.9; eight groups fall between 99.0 and 103.5.

One group with which Wagner (*op. cit.*) deals comprises Rodenwaldt's European-Indonesian hybrids on the island of Kisar. (Rodenwaldt himself finds significantly higher coefficients of variation for the Mestizos than for the pure Kisarese, but these are all in characters in which the n of the Kisarese group is only 15; and the present investigation has already shown how small groups may have extremely low standard deviations, a mathematical rather than a biological phenomenon.)

Wagner, using the mean coefficient of variation compounded of 13 ill-assorted characters, mostly indices, compares the Mestizos to a sample from Baden, concluding that: "The only thing that can be said is that these hybrids undoubtedly show a greater average variability than a European population of not particularly pure race."

The mean sigma ratio for the Mestizos given in Table 3, although the component characters are different in this case, shows that this deduction is not valid, and that the Mestizos are not more variable than typical European groups (*i.e.*, Norwegians), or any other typical racial group.

Also presented in Table 3 are Sullivan's "pure" and "half-blood" Sioux. In this case the mean sigma ratios call for no comment.

From the Jamaican material of Davenport and Steggerda given in Table 4 it appears that the hybrid "Browns" are intermediate in the

[5]The mean sigmas, as used in calculating the following sigma ratios, have been rounded off to a single place of decimals.

TABLE 3

Standard Deviations and Sigma Ratios of Hybrid and Pure Groups.

	MESTIZOS OF KISAR (79) (RODENWALDT)		PURE SIOUX (539) (SULLIVAN)		HALF-BLOOD SIOUX (77) (SULLIVAN)	
	S. D.	Sigma Ratio	S. D.	Sigma Ratio	S. D.	Sigma Ratio
Stature	5.86	101.0	5.64	97.2	6.81	117.4
Head length	6.10	98.4	6.16	99.4	7.12	114.8
Head breadth	4.26	81.9	5.39	103.7	5.04	96.9
Minimum frontal	5.06	103.3	...	...	...	...
Bizygomatic	5.07	95.7	5.45	102.8	5.49	103.6
Bigonial	5.83	100.5	...	...	...	...
Face height	6.12	95.6	6.39	99.8	6.36	99.4
Nose height	...	...	3.94	103.7	3.55	93.4
Nose breadth	3.14	108.3	3.22	111.0	3.04	104.8
Cephalic index	3.44	101.2	3.20	94.1	2.64	77.6
Cephalic-facial index	...	...	3.22	92.0	3.23	92.3
Facial index	5.08	99.6	4.84	94.9	5.28	103.5
Nasal index	7.33	94.0	7.05	90.4	7.08	90.8
Mean		98.1		99.0		99.5

degree of their general variability between the Blacks and the Whites. The Whites have a high mean sigma ratio, which would be still higher but for the low sigma of the nasal index. Their high variability is probably due to heterogeneity of their European origin. This unhomogenous parentage on one side, rather than their hybrid nature, is also perhaps the best explanation of why the Browns are more variable than the Blacks.

Other examples of a like nature (*e.g.,* Williams' Maya-Spanish crosses) might be adduced without showing highly variable hybrids. Fischer's "Rehobother Bastards" have not been quoted in this paper, partly because such small series have erratic sigmas, and partly because Fischer gives constants for very few of the characters on our list of mean sigmas.

TABLE 4

Standard Deviations and Sigma Ratios of Jamaica Material (Davenport and Steggerda)

	BLACKS (54)		BROWNS (93)		WHITES (50)	
	S. D.	Sigma Ratio	S. D.	Sigma Ratio	S. D.	Sigma Ratio
Stature	6.76	116.6	6.75	116.4	6.86	118.3
Head length	6.84	110.3	6.44	103.9	6.99	112.7
Head breadth	4.42	85.0	5.16	99.2	6.25	120.2
Nose height	4.09	107.6	3.55	93.4	4.48	117.9
Nose breadth	2.75	94.8	3.44	118.6	2.56	88.3
Cephalic index	2.76	81.2	2.80	82.4	3.42	100.6
Facial index	5.53	108.4	5.20	102.0	6.33	124.1
Nasal index	9.20	117.9	9.80	125.6	5.97	76.5
Mean		102.7		105.2		107.3

None of the groups so far considered, hybrid or otherwise, has exhibited a mean sigma ratio exceeding 100 by more than a slight amount, with the exception of the Jamaican "Whites". Weninger's West African Negroes give a sigma greater than 100 in 13 out of 14 characters, with a mean of 109.1. The reason for this high figure probably lies in the selection of the series, which consists of 100 men drawn from a variety of tribes through Senegal and Nigeria, an enormous area to include in a single sample. A group of 187 from the island of Tanna in the New Hebrides gives the very high mean ratio of 118.5. Although the area is in this case restricted, the population of Tanna is a conglomerate, since it includes relatively pure Melanesian types as well as Polynesian-Melanesian hybrids, and the wide range covered by such a combination produces consistently high standard deviations.

SUMMARY

1. The standard deviation in anthropometric series of less than 50 observations is highly affected by the individuals furthest removed from the mean, and is apt to be very high or very low; it is apparent that it

passes almost completely under the influence of the laws of probability and has little or no biological significance. The average size of the standard deviation has a tendency to increase with the size of the samples, and to become less erratic, i.e., to have a smaller standard deviation of its own.

2. To facilitate comparisons of group variability, etc., the use is suggested of a "sigma ratio" between any given standard deviation and the mean sigma for the character, this being a fixed figure, the arithmetic mean of a series of sigmas from various racial groups of 50 or more males assembled from the literature.

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MORTALITY AMONG PATIENTS WITH PARANOIA OR PARANOID CONDITIONS

BY BENJAMIN MALZBERG, PH. D.

New York State Department of Mental Hygiene

IN A detailed study of mortality among patients with mental disease in New York State it was shown that such patients had a rate of mortality in excess of that of the general population in the ratio of 4.8 to 1. The patient population and the general population had standardized rates of 64.9 and 13.5, respectively, per 1,000 population aged 15 years and over. (1) It was shown that this excess was true in the case of all the principal groups of psychoses, though there was much variation in this respect. The rates of mortality were highest in the organic groups; *i.e.*, psychoses with brain tumor, senile psychoses, psychoses with cerebral arteriosclerosis and general paresis. They were lowest in the functional groups, such as psychoses with psychopathic personality and dementia praecox. (2)

One of the groups of exceptional interest with respect to mortality is the paranoid. This is not a large group in comparison with patients with dementia praecox or manic-depressive psychoses, for example, but it is important clinically. Our group of patients with paranoia is not synonymous with all patients showing paranoid trends or dispositions. The latter symptoms may be found in several distinct clinical types of mental disease. According to the official classification of mental disorders adopted by the American Psychiatric Association and utilized in the New York civil State hospitals, patients with paranoia or paranoid conditions do not include "the deteriorating paranoic states (dementia praecox) and paranoid states symptomatic of other mental disorders, included under the organic brain disease, toxic and other groups. In the present group are to be classified those cases showing fixed suspicions and ideas of persecution logically elaborated for the most part after a false interpretation of an actual occurrence has been made. The emotional reactions are consistent with the ideas held and such persons are prone to take action against their persecutors, rendering them

dangerous" (3). This rigorous delimitation of the paranoid group is significant as it will be shown to have an important effect upon the mortality rate.

On April 1, 1930 there were 1,183 patients with paranoia or paranoid conditions in residence in the New York civil State hospitals, of whom, 429, or 36.3 per cent, were males, and 754, or 63.7 per cent, females. Their average age was 62.2 years, an age exceeded only by that of patients with senile psychoses and psychoses with cerebral arteriosclerosis. Males and females had average ages of 61.3 and 62.8 years, respectively. There was comparatively little variation, 61.6 per cent of the patients in this group of psychoses being between 50 and 69 years of age.

Such an age distribution would ordinarily be associated with a high death rate, but the chronic condition so prevalent among paranoids exerts a counterbalancing influence. The chronic condition of these patients is made evident by the fact that patients with paranoia or paranoid conditions, who died in the New York civil State hospitals during the year ended June 30, 1930 had had an average hospital residence of 17.8 years, compared with an average of only 6.3 years for all the patients who died in that year. Furthermore, 47.7 per cent of the patients who died in 1930, had been in the hospital less than one year, and more than half had been resident for 15 years and over. As will be seen, this results in a relatively low death rate in this group of psychoses.

During the three years ended June 30, 1931 there were 173 deaths among patients with paranoia, of which 75, or 43.4 per cent, occurred among males, and 98, or 56.7 per cent, among females. The average age at death was 69.3 years, an age exceeded only by that of patients with senile psychoses. The average of all patients dying in the same period was 60.1 years. The female paranoids died at an older age than the males, the averages being 70.7 and 67.5 years for females and males, respectively. The total annual exposures during the three years included in the experience were 1,300.5 years for males, 2,290.0 for females, and 3,590.5 for both sexes combined.

The average annual death rate was 48.2 per 1,000 exposures, which gave a ratio of 4.1 to 1 in comparison with the death rate of the general population of the State of New York. Only in dementia praecox, psychoses with psychopathic personality and psychoses with mental deficiency, do we find lower crude death rates than in paranoia. (4)

Male paranoids had an average annual death rate of 57.7 per 1,000 exposures, giving a ratio of 4.6 to 1 in comparison with the death rate of the general male population. Among female patients there was a death rate of 42.8 per 1,000 exposures, with a corresponding ratio of 4.0 to 1 to the death rate of the general female population.

Death rates among the patients are shown by broad age groups in Table 1.

TABLE 1

Ages of Patients With Paranoia or Paranoid Conditions Dying in the New York Civil State Hospitals During the Fiscal Years 1929-1931 Inclusive, and Average Annual Death Rates per 1,000 Exposures

AGE (years)	NUMBER OF DEATHS			PER CENT OF TOTAL DEATHS			AVERAGE ANNUAL DEATH RATE PER 1,000 EXPOSURES		
	Males	Females	Total	Males	Females	Total	Males	Females	Total
35-44	2	1	3	2.7	1.0	1.7	23.1	6.8	12.9
45-54	8	8	16	10.7	8.2	9.2	28.5	23.3	25.7
55-64	20	18	38	26.7	18.4	22.0	49.4	23.8	32.8
65-74	23	35	58	30.7	35.7	33.5	63.6	48.9	53.8
75 and over..	22	36	58	29.3	36.7	33.5	141.7	117.5	125.7
Total	75	98	173	100.0	100.0	100.0	57.7	42.8	48.2

The death rate rose steadily from a minimum of 12.9 per 1,000 exposures at 35 to 44 years to a maximum of 125.7 at 75 years and over. Among males the death rates rose from 23.1 at 35 to 44 years to 141.7 at 75 years and over. The females showed a similar trend from a minimum death rate of 6.8 at 35 to 44 years to a maximum of 117.5 at 75 years and over. The male death rate exceeded the female rate in every age interval.

The death rates of the patients are compared with those of the general population in Table 2, which also gives the ratios of the two sets of death rates at corresponding ages.

TABLE 2

Death Rates by Age Groups, Among Patients with Paranoia or Paranoid Conditions, in the New York Civil State Hospitals, and in the General Population of the State of New York, in 1930, With Ratios of Corresponding Death Rates

AGE (years)	AVERAGE ANNUAL DEATH RATE AMONG PATIENTS WITH PARANOIA*			ANNUAL DEATH RATE IN GENERAL POPULATION OF NEW YORK STATE†			RATIO OF DEATH RATE AMONG PATIENTS TO THAT OF THE GENERAL POPULATION		
	Males	Females	Total	Males	Females	Total	Males	Females	Total
35-44	23.1	6.8	12.9	7.6	5.6	6.6	3.0	1.2	2.0
45-54	28.5	23.3	25.7	16.0	11.3	13.7	1.8	2.1	1.9
55-64	49.4	23.8	32.8	32.2	24.9	28.5	1.5	1.0	1.2
65-74	63.6	48.9	53.8	62.7	52.9	57.6	1.0	0.9	0.9
75 and over .	141.7	117.5	125.7	144.0	134.3	138.5	1.0	0.9	0.9

*Per 1,000 exposures. †Per 1,000 population.

At 35 to 44 years the patient death rate bore a ratio of 2.0 to 1 to that of the general population. The ratio decreased steadily with age. At 65 to 74 years and again at 75 years and over the death rates of the general population actually exceeded those of the patients, in the ratio of 1.1 to 1.

Among males the ratio decreased steadily from a maximum of 3.0 to 1 at 35 to 44 years. At 75 years and over the death rate of the general population was in excess in the ratio of 1.02 to 1. Among females the ratio rose from 1.2 to 1 at 35 to 44 years to 2.1 to 1 at 45 to 54 years, and then decreased to minima at 65 to 69 years and 75 years and over. In the latter intervals the death rates of the general population were in excess in the ratio of 1.1 to 1. The male ratio was generally in excess of that of the females.

It is evident from these results that patients with paranoia had, on the whole, higher death rates than the general population. This is especially true of the males. But it is also evident that the direct comparisons of summary 'crude' rates shown earlier in the discussion are unjustified. On the latter basis it appeared that the death rate of the

paranoids exceeded the general death rate by more than 300 per cent. But this is spurious, resulting from the fact that there were no paranoids under 35 years of age, in which intervals mortality rates are relatively favorable. Comparing the specific rates of mortality at 35 years and over, however, it is clear that the patients with paranoia have, in the main, a heavier mortality than the general population, but the excess is much less than that indicated by the crude rates. We may standardize the rates in an approximate manner by applying the specific rates to the corresponding population of the State of New York, aged 35 years and over, as shown by the Federal census of April 1, 1930. The results are shown in Table 3.

TABLE 3

Death Rates Among Patients With Paranoia or Paranoid Conditions and in the General Population of the State of New York, 1930

	MALES		FEMALES		TOTAL	
	Crude	Standardized	Crude	Standardized	Crude	Standardized
Patient population (A)	57.7±4.4	39.3±3.7	42.8±2.9	22.3±2.1	48.2±2.4	31.8±2.0
General population (B)	12.6±0.03	23.7±0.1	10.7±0.03	18.9±0.1	11.7±0.02	22.3±0.04
Ratio of (A) to (B)	4.6	1.7	4.0	1.2	4.1	1.4

Standardization decreased the patient death rates and increased those of the general population. The former had a rate of 31.8 per 1,000 population, giving a ratio of 1.4 to 1 in comparison with the death rate of the general population, a marked reduction from the ratio based upon crude rates. Males had a standardized death rate of 39.3 which exceeded the corresponding rate of the general male population in the ratio of 1.7 to 1. Female patients had a standardized rate of 22.3 giving a ratio of 1.2 to 1 to that of the general female population, though the excess is not statistically significant.

Patients with paranoia have a very favorable death rate in comparison with all mental patients, however. The latter had a standardized death rate of 64.9 per 1,000 population aged 15 years and over. In view

of the different age limits in the two groups, this rate cannot be compared directly with that of patients with paranoia, but the contrast is interesting and significant, nevertheless, since the former rate is affected favorably by the inclusion of younger age elements. It is a fair inference that the death rate of patients with mental disease is more than twice that of the paranoid group.

Summarizing we may say that patients with paranoia or paranoid conditions have a lower death rate than most other groups of patients with mental disease, but their mortality remains somewhat in excess of that of the general population. One would probably have anticipated a higher rate of mortality among the paranoid group than that actually found. The relatively favorable status results from the fact that hospital cases of paranoia represent a group composed largely of chronic patients. Hospital deaths occur principally among newly admitted patients who are in an acute condition. Furthermore the official classification of patients with mental disease excludes from the paranoid group many with accompanying physical disorders which result in a high mortality rate. The hospital group of paranoids is thus a highly selected group with respect to physical symptoms, a fact which undoubtedly explains their relatively low mortality in comparison with that of all mental patients.

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PHYSIOLOGIC STERILITY OF ADOLESCENCE

BY C. A. MILLS, M.D., PH.D. AND CORDELIA OGLE, PH.D.

(From the Laboratory for Experimental Medicine, University of Cincinnati)

ALTHOUGH much discussion has waged around the question of adolescent sterility, little real investigation has been made. The freedom with which the adolescent youth of many races indulge in promiscous sex relations, with only rare conception resulting, must indicate a relative sterility. With the primitive Igorots and Bontocs of Northern Luzon sexual intercourse among the adolescent youth is unbelievably free, but rarely does conception occur before regular marriage mating has taken place. What is there in the knowledge of these and other primitive people that enables them to secure legitimate marriage (according to whatever standards they may have) before the onset of fertility? Often several years elapse with this free and sterile premarital intercourse.

Hartman (1), studying the problem in monkeys, found the onset of fertility to be closely related to general body development. Regardless of how early the menses began, the monkeys were not fertile until reaching a certain body size and weight. In his stock, menstruation began when body weight was around 3350 grams, but ovulation and ability to conceive were delayed until a weight of about 5000 grams was reached. In time this represented practically a year's lag in fertility after the onset of the menses. Vignes (2) quotes Paul Godin as stating that the fertility of sexual maturity comes about five years after the onset of the menses in girls, but no statistics are given.

There have been other observations suggesting a real physiologic basis for adolescent sterility, but it has seemed desirable to put the matter on a more definite foundation. Hence we are presenting statistics on menarche, marriage and first delivery ages, collected in widely separated locations and on different races of people. Data from white mice closely verify the human findings, completing the picture of adolescent sterility. In the present communication we shall present data on girls of various races, and on white mice showing that adolescent sterility is

a very real phenomenon, that its duration bears a definite relationship to the age at which the menses start and also to the urge that leads to marriage or illegitimate births.

During the year 1935 one of us had occasion to collect statistics on the onset of the menses from hospital obstetrical records of widely different regions. Menarche, marriage and first delivery ages for mothers were taken from the records and form the basic material of this article as it concerns human beings. In every case the data relate to persons from the lower economic levels of society, being in the main charity patients in general hospitals.

Table 1 presents the data in summarized form. At Cincinnati obstetrical records of 1924 yielded 33 cases of illegitimate negro first births in which the lag from mean menarche age of the mother to mean first delivery age was found to be 4.64 years, or 3.89 years to the time of conception. Fifty six Richmond negro girls showed a slightly longer lag from mean menarche age to first conception, 4.00 years. The lag for 72 negroes in the Panama Canal Zone was still greater, 4.47 years, and for 22 Manila Filipinos, 6.33 years. It is to be noted that these increasing lag periods in the onset of fertility were accompanied by a later and later menarche age. We have elsewhere (3) remarked on the fact that the menses *do not begin earlier* in tropical warmth than in cooler temperate regions, but that growth and pubertal development are most accelerated in the stimulating, stormy, temperate regions, and are most retarded in the depressing moist heat of the tropics. The data on which the belief in early tropical maturity was based came from India, where prepubertal marriage and sexual intercourse are common. Statistics from peoples not given to childhood matings show a delayed development in tropical heat. A separate paper is being prepared on this subject and will appear shortly.

The data on Chinese mothers at Hongkong furnish a point of interest in connection with this factor of possible stimulation of fertility by intercourse. Illegitimacy is almost unknown among the Chinese, marriage takes place early, and a real desire for children precludes any contraceptive measures or inhibitions. Although the menses begin later in the Hongkong Chinese than in Manila Filipinos, the first conception takes place 1.3 years earlier. This earlier age at first conception in the Chinese may possibly be due to the greater regularity and frequency of sexual intercourse in the married state than in the unmarried Filipinos. Comparison of the married Filipinos with the Chinese is of

TABLE 1

Menarche, Marriage and First Birth Data

CITY	SUBJECTS	DATE OF RECORDS	NO. OF CASES	MOTHERS' AGE IN YEARS AT MENARCHE	MARRIAGE	FIRST DELIVERY	LAG IN FERTILITY (Menarche to first Conception)
							Years
Cincinnati	Negroes, unmarried	1924	33	13.44±0.13	.....	18.08±0.28	3.89
	White } married		131	13.68±0.08	19.09±0.17	20.48±0.19	6.05
	Colored }						
Richmond, Va.	Negroes, unmarried	1934	56	13.46±0.09	.....	18.21±0.19	4.00
	Negroes, married		52	13.75±0.17	19.63±0.17	21.60±0.43	7.10
Panama	Negroes, unmarried	1934	72	14.04±0.09	.....	19.26±0.20	4.47
	Negroes, married		56	14.05±0.14	21.01±0.27	21.93±0.32	7.13
Manila	Filipinos, unmarried	1934	22	14.73±0.16	.....	21.82±0.50	6.33
	Filipinos, married		84	14.58±0.12	20.49±0.30	21.88±0.30	6.55
Hongkong	Chinese, married	1934	284	15.82±0.06	18.27±0.11	21.00±0.12	4.97

TABLE 2

Mothers' Age at Illegitimate First Birth, by Menarche Age

AGE AT MENARCHE	NO. OF CASES	AGE AT ILLEGITIMATE FIRST BIRTH	LAG IN FERTILITY
		Negro	
Years		Years	Years
11-11.9	9	18.11 (aver.)	5.9
12-12.9	54	18.02±0.21	4.8
13-13.9	61	17.78±0.18	3.5
14-14.9	55	19.14±0.19	3.9
15-15.9	19	19.71±0.38	3.5
16-16.9	7	20.21 (aver.)	3.0
17-17.9	1	24.5	...
		White	
11-11.9	6	19.83 (aver.)	7.6
12-12.9	15	18.30±0.66	5.1
13-13.9	27	19.54±0.27	5.3
14-14.9	20	19.85±0.35	4.6
15-15.9	4	20.5 (aver.)	4.3
16-16.9	8	20.25 (aver.)	3.0
17-17.9	2	21.0 (aver.)	2.8
		Both Races	
11-11.9	15	18.83±0.49	6.6
12-12.9	69	18.22±0.21	5.0
13-13.9	88	18.32±0.16	4.1
14-14.9	74	19.35±0.17	4.1
15-15.9	23	19.85±0.34	3.6
16-16.9	15	20.23±0.39	3.0
17-17.9	3	22.2 (aver.)	...

little value because of the later marriage age at Manila. It is a striking coincidence, however, that the age at first delivery in Filipino mothers is practically the same whether they be married or unmarried. Sex relationships in Manila are perhaps as free from social or moral restraint as anywhere on earth, so that we may be justified in accepting this lag in conception (6.33 years after menarche) as a real expression of physiologic sterility. The quickening in development that comes under more stimulating climates is accompanied by a shortening of this sterile period.

Table 2 presents another phase of the matter. Classifying the illegitimate first deliveries by age of the mother at menarche, it is found that earliest delivery comes with a 12 or 13 year menarchial age and that first childbirth is delayed more and more the later in life the menarche occurs. This phenomenon holds equally true for white or colored girls or for both groups combined. The sterile lag period from menarche to first conception, however, exhibits a steady shortening as we go from earlier to later menarchial age. This would seem to mean that less variability exists in the age for onset of fertility than in that for the onset of the menses. There must be kept in mind, though, the not improbable possibility that this is only a seeming relationship, dependent perhaps on lack of matings at the earlier ages rather than on lack of biologic fertility.

TABLE 3

Marriage Age, by Menarche Age. Richmond and Cincinnati Data Combined.

AGE AT MENARCHE	NO. OF CASES	AGE AT MARRIAGE	LAG IN MARRIAGE AFTER MENARCHE
10 to 10.9 years	5	18.9 (aver.)	8.4
11 " 11.9 "	15	20.30+0.87	8.8
12 " 12.9 "	49	19.15+0.29	6.7
13 " 13.9 "	93	18.60+0.18	5.1
14 " 14.9 "	42	19.17+0.25	4.7
15 " 15.9 "	28	19.93+0.50	4.4
16 " 16.9 "	11	20.68+0.84	4.1
17 " 17.9 "	5	22.1 (aver.)	4.6
18 " 18.9 "	3	22.5 (aver.)	4.0

Table 3 throws further interesting light on the problem in its analysis of the relation between menarche and marriage age. Here is seen precisely the same relationship as was found to exist between menarche and illegitimate first births, with earliest marriage taking place in the group with a 13 year menarche age. And again, although the marriage age comes progressively later as the age at menarche is more delayed, the lag period from menarche to marriage is of much the same order as the lag between menarche and illegitimate first birth. One might offhand say that the marriages show this relationship to the menses and illegitimate births because of the first pregnancy really antedating the marriage ceremony. Such a possibility has been excluded, however, by including in the illegitimate group all those first births that occurred within about 8 months after marriage. The fact that, among the American groups, the first births among married mothers came at a considerably later age than did the illegitimate first births would seem to indicate the working of a higher degree of inhibition in the former group, thus reducing the likelihood of pre-marital pregnancy.

With marriage age, however, showing much the same relationship to age at menarche as does the age of mothers at illegitimate first births, one wonders whether the onset of true fertility in these groups is not acting as the determining urge to legitimate mating. In other words, is it true biologic fertility which lies behind and largely times the mating for "procreative" intent? With the married mothers, first delivery in the great majority of cases comes in the year after marriage, probably indicating a real desire for the first child. Hence the relationship found here between marriage and menarche age very likely has the same significance as has that for the relation of menarche to illegitimate first birth. The onset of biologic fertility, then, seems to determine either marriage or extra-marital conception.

Such data on human beings would not be considered as conclusive evidence for the above statements without corroboration on experimental animals under controlled conditions. We have no records of the timing of intercourse in the human cases cited here, hence much of the discussion must remain problematical. It so happens, however, that animals give definite evidence of this same adolescent period of sterility, as is shown in Table 4 for white mice.

Mice, grown from a single stock, were divided into four groups. One group was kept in the control breeding room at 70-80°F., another was placed in a special room kept at 64-68°F., and a third placed in a

TABLE 4

First Oestrus and First Delivery Data on White Mice

DESCRIPTION OF GROUP	NO. OF CASES	AVERAGE AGE AT FIRST OESTRUS	AVERAGE AGE AT FIRST DELIVERY	LAG IN FERTILITY (First Oestrus to First Conception)
		days	days	days
Breeding room controls	20	33	84	31
Cold room immigrants	11	36	88	32
Cold room descendants	25	33	79	26
Hot room immigrants	31	37	94	37
Hot room descendants	9	46	...	...
Change group immigrants	26	35	138	83

Gestation period was taken as 20 days. "Immigrants" in each case were young of control stock transferred to the indicated environment. "Descendants" were from mothers already adapted to the indicated environment.

hot room kept at 88-92°F. and about 70 per cent relative humidity. A fourth group was shifted daily between the hot and cold rooms, spending 12-16 hours of each day in the moist heat. In every case pregnant mothers were allowed to deliver their young in the control room and kept there with the litter until the young were 21 days old. This was necessitated by the fact that the mothers usually kill the young if left in either the heat or cold.

From Table 4 we see the first oestrus, corresponding to the human menarche, beginning at 33 days of age in the control room group, at 36 days for the young of control room mothers placed in the cold room at 20 days of age, at 33 days for the cold room young of cold room mothers, and at 35 days for the young of control room mothers shifted daily from hot to cold. Young placed in the moist heat, however, show a definite and striking delay in the age of first oestrus. Young of control room mothers, placed in the heat at 20 days of age, began the oestrus at 37 days of age, but the young of hot room mothers were delayed to *46 days,* confirming our earlier statements on the later menarchial age for girls of native tropical stock.

In these same groups of mice records were also kept of the age at first delivery, as shown in the second column of data in Table 4. Fertility in the offspring of hot room mothers was so low that too few conceptions were obtained to have any significance. The young of control room mothers, placed in the heat, exhibited a definite delay in first delivery, while the young placed in the group shifted daily from heat to cold were markedly inhibited in their onset of fertility, apparently unable to adapt to the more intense stimulation.

Considering the gestation period for mice at an average of 20 days, we see the lag in fertility after the first oestrus to be 31 days for the control room mice, 30 days for the control room young placed in the cold room, *only 26 days* for the cold room young of cold room mothers and *37 days* for the young placed in the heat. With the young shifted daily from heat to cold, fertility was almost destroyed, but in 26 females that did conceive the first conception came after an average lag of *83 days* from the time of the first oestrus.

Here we have confirmation of the human findings in several respects. First, it is certainly true that a period of physiologic sterility does exist in most individuals, either human or animal, during adolescence. In the groups of mice only about 5 percent conceived with each of the first three oestrous cycles, the peak in conceptions coming with the 6th or 7th cycle. Adaptation of animals to depressing moist heat delays the onset of the oestrous cycles and prolongs the lag in fertility. Adaptation to a more stimulating environment speeds up the whole sexual process, just as is shown by the data on human beings.

One further point of interest lies in the striking suppression of fertility seen in mice when they are exposed to too great temperature variability. Many of them never conceive, although matings are freely accomplished, and in those that do become pregnant fertility does not manifest itself until late in life. Usually only a single litter is obtained from these females. Perhaps these animal findings in a way explain the very low fecundity exhibited by the natives of the Australian desert regions, who live without protection under extremely wide diurnal fluctuations in temperature. No doubt such bodily exhaustion also accounts for much of the sterility seen in the cooler stormy portions of North America.

The data on mice were insufficient for grouping by age at first oestrus to see if the lag period in fertility decreases with later onset of

the cycles. By using three large groups (excluding the mice subjected to wide daily change) the following figures were obtained, but are probably of little significance except that they point in the same direction as do the human findings.

With first oestrus at	*Lag in fertility was*
24-32 days	33.4 days
33-41 "	32.5 "
42-50 "	29.5 "

CONCLUSIONS

Data from widely separated groups of people give definite evidence of physiologic sterility during adolescence.

This sterile period shortens progressively as the menarche is delayed. Menarche and the onset of fertility come earliest in regions of greatest climatic stimulation and are most delayed under tropical moist heat.

White mice, under controlled conditions of artificial climate, supply evidence definitely corroborating the data from human beings. In them stimulating cold produces a marked quickening in sex development of the second generation, while moist heat results in depression of function and slower development.

Either marriages or illegitimate first births show a tendency to come earliest in those girls whose age at menarche is near the mean for the whole group. As menarche occurs earier or later than the mean age, marriage or illegitimate first birth is delayed.

The similarity of the relationships or marriage and illegitimate first births to menarche age would seem to point to an onset of fertility as the dominant force determining the time of marriage or of illegitimate conception.

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NOTES

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THE object of this Bureau is collection on as wide a scale as possible of material dealing with human Genetics. Later, the tasks of analysis of material and distribution of the information available will be added.

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RECENT LITERATURE

USEFUL IN THE STUDY OF HUMAN BIOLOGY

In this department will be noticed the books and reprints which are currently received at the Editorial Office. Publications intended for notice here should be addressed to Dr. Raymond Pearl, Department of Biology, School of Hygiene and Public Health, 1901 East Madison Street, Baltimore, Maryland, U. S. A.

I. EVOLUTION AND GENETICS

1. *Evolution—General Principles*

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